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C U R I O U S
R E M A R K S
A N D
O B S E R V A T I O N S

Extracted from the
HISTORY AND MEMOIRS
O F T H E
Royal Academy of Sciences at *Paris*.

V O L. II.
Which concludes the GENERAL PHYSICS.

B Y
Dr. *PETER TEMPLEMAN*.

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DR. PETER TEMPLEMAN

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INDICIV

P R E F A C E.

BY the advice of some gentlemen, who have done me the honour to approve of my former volume, and who are themselves the brightest ornaments of the Republic of Letters, I have enlarged my original design, and shall extract from the history and memoirs of the Academy, not only such things as are new and un-observed by others, but likewise whatever in my own judgment is worthy of the public attention.

Enquiries of this nature which tend to enlarge our ideas of the Divine Power, to convince us of the constant superintendency of Providence, and to display the immensity of the works of the Creator, are particularly useful at this time, when such bold and im-

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P R E F A C E.

pious attacks are made on the very vitals of all religion: Observations of nature will necessarily lead a contemplative mind to the acknowledgment and adoration of the author of nature.

I shall next proceed on the *anatomical* part of my work, and as there are many curious dissections of the brute animals, particularly of the wild kind from the French king's Ménagerie, I shall divide my subject into *human* and *comparative* anatomy; but for the greater ease and benefit of my readers, whatever relates to each science shall be contained in distinct volumes, and may be purchased separately.

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E R R A T A in Vol. I.

Page 193, line 22, for 8 read 7.
Page 252, line 5, read senior professor.

In Vol. II.

Page 192, line 21, after effect put a comma.

GENERAL PHYSICS.

IN vain would nature have displayed such wisdom in the frame of animals, if after having formed with *wonderful art* their internal parts, she had neglected to defend them from surrounding bodies: The too rough touch of those bodies would soon have destroyed such delicate springs, such tender fibres, as give motion to every vital wheel, and on which the whole animal machine is hung. But indeed nature (*or, to speak more properly, Divine Providence*) hath taken care to clothe these delicate parts with divers coverings, that cannot easily be injured by the bodies which surround them; not only hath she included them in an outward skin more compact and more solid than the other membranes, but she hath moreover commonly covered this outward skin with hairs, feathers, scales, or shells: These are a kind of little bulwarks, if I may be allowed the expression, under the covert of which the animal machines can sustain the efforts of most of those bodies which continually rub, impel,

Of the formation, growth, and colours of the shells of animals, by Mr. De Reaumur.

or rudely strike them. The attention of nature hath even gone so far as to proportion the strength of these defences to the weakness of the internal parts; I mean, that the animals, who by their figure or softness of substance give more *bold* to the bodies that surround them, have in requital stronger coverings; thus we see that shells cover those animals whose substance is very moist and soft, and whose figure is almost flat or spiral, which by this double inconvenience would be in danger of being cut and torn by the earth, the sand, or stones on which they crawl. How many different species of animals doth nature preserve on the earth, in rivers, and in seas, by means of these shells? With how much art doth she seem to have laboured them? One would think that she took pleasure and sported, as it were, with varying their figures, structures and colours; insomuch that the greatest part of *those* who have a delicate sense of the beauties of nature have employed the utmost pains in collecting the greatest number they have been able, each new shell affording a fresh incitement to their curiosity: Their cabinets contain but a part of those that adorn the universe, yet they have enough to excite the admiration of *those* who have the understanding to admire.

But one would think that they had confined themselves to gazing *only* on this beautiful

teous work ; no one, at least that I know of, hath explained in what manner it is produced ; so that having received no instructions on this subject from authors, I have *consulted* nature herself by divers experiments ; and it is by relating the *answers* of that divine oracle, that I am going to shew how the formation and growth of shells are accomplished.

Although it might appear more regular to explain first, in what manner the shells of animals are formed, before I speak of their growth ; yet I shall here pursue a contrary order : I shall begin with explaining in what manner they *grow*, which it is a more easy task to discover, and which will be sufficient to give light into the manner of their *formation*, that may be considered as the first *step* towards growth.

A body may grow in two different manners, or, to express myself more clearly, the little pieces of matter that come to unite themselves to *those* which already compose the body, and that by this means increase its bulk, may be added to it in two different ways ; *either* these pieces do not attach themselves to those that already constitute the body, 'till they have passed *through* that body, have been prepared in it, and in some measure rendered proper for occupying the place whither they are conveyed ; this is what is commonly called *growing by vegetation*,

and in the schools *growing by intussusception*.

It is *thus* that the sap mounts up in plants by divers small vessels, which, after having prepared it in a certain manner, convey it to different parts of the plant, where it fastens itself and increases, by consequence, the bulk of the plant. Thus likewise a certain portion of blood having been conveyed by the arteries to the extremities of the body of an animal, bestows something on the fleshy parts, and increases their size.

Or, the second way of *growing* is, when the parts which augment the bulk of a body are applied to it without having received any preparation in the body itself, and this is called *growing by apposition*, or in terms of the school by *juxtaposition*. All those artificial plants which we owe to the skill of the chemists, such as arbor dianaë, arbor martis, as well as all chrySTALLISATIONS, salts, &c. grow in this manner.

The growth of shells must be effected in the one or other of the preceding manners. Those that have made every thing *vegetate*, even stones; would probably be very far from suspecting that shells, which are worked with so much art, could be produced by mere *juxtaposition*. The analogy that appears to be between shells and bones (for might they not be considered as external bones?) would seem to confirm their opinion,

nion, since bones truly vegetate. But such conjectures are of no efficacy in sound philosophy; experiments alone made on the things themselves that are the subject of enquiry, can justly serve for a foundation to our reasonings: They alone can point out the way that it hath pleased nature to take in order to arrive at her end: It is by means of these experiments that we shall see afterwards that shells are produced by a *simple apposition*.

Although I have made experiments only on some species of shells, yet I think I have a right to *assume* an explanation of the growth and formation of shells in general. That nature makes use of general methods for producing such works is sufficiently known. Would it not be thought sufficient for a philosopher to have explained how a plant grows, in what manner nutrition is performed in an animal, in order to conclude from thence, or rather that the whole philosophic world should conclude with him, that it is *thus* all plants grow, that nutrition is performed in the same manner in all animals? After it had been demonstrated that the blood circulated in *man*, who ever doubted but it circulated in all animal machines?

I shall therefore content myself with relating the experiments I have made on divers species of terrestrial snails.

As the animal that filled exactly its shell is growing, it necessarily follows, that the same shell doth not continue sufficiently large to cover it intirely, or, in other words, that a part of the surface of the body of the animal must become naked; the part, which is thus destitute of shell by the growth of the animal, is always *that* which is the nearest to the mouth of the shell; for the body of the animal can extend itself only that way. All the animals that inhabit shells *turned* in a spiral form, such as snails, can be extended only about the head, where the mouth of the shell is; whereas the animals of bivalved shells, such as muscles, may be enlarged throughout their whole circumference. Now in all the species of shell-fish, it is that very part of the body, which is *bare* by the growth of the animal, that occasions the shell to grow. I shall explain the mechanism on which this growth is founded.

It is a necessary effect of the laws of motion, when liquors flow through vessels, that the small parts of these liquors, or the little heterogeneous bodies mixed with them, which on account of their figure or their little solidity, in comparison to their surface, have less motion than the other parts, remove from the center of motion, or place themselves near the sides of those vessels. It even often happens that these small parts *fasten*
them-

themselves to the internal surface of those vessels, when they are sufficiently viscous for that purpose. Pipes that convey water to reservoirs furnish us with examples of it. We commonly find, when they are opened, the internal surface covered with a little crust of viscous matter: It is likewise observed that those pipes, through which *certain* waters flow, have a *stony* incrustation. It is moreover certain that the liquors which flow in these pipes press upon the sides in all directions, or (which is the same thing) that they urge the stony and viscous parts of the crusts that we have been speaking of against the sides; insomuch that if these pipes were perforated, like sieves, with an infinite number of little holes, of a suitable figure for giving passage to these little viscous and stony bodies *alone*, they would escape from the pipes, and place themselves on their external surface; where they would form the same crust as one sees on their internal surface, with this difference alone, that the *crust* might become much more solid and even thicker, as it is much less exposed to friction from the liquor than *that* which is formed in the inside of the pipe. The growth of shells is the work of a like mechanism; the external surface of that part of the body of the animal, which is too much enlarged to be covered by the antient shell, is full of a prodigious number of pipes

or tubes, in which circulate the liquors necessary for the nutrition of the animal; a great number of little pieces of a viscous and stony matter are mixed with these liquors, but as they are less fluid than the particles of the liquors with which they flow, they are thrown to the sides of these vessels, which being full of an infinite number of pores, proper for giving them passage, these little pieces of a viscous and stony matter easily escape from the vessels that contained them; for they are continually pushed against the sides by the liquor that fills them; and they go to place themselves on the external surface of these tubes, or *rather* on the whole surface of the animal which is not covered by the shell, whither they arrive with so much the more ease, as all the pores give them a free issue, whereas several of the pores on the rest of the body may be stopped up by the shell with which it is invested. These little pieces of a viscous and stony matter being arrived at the extreme surface of the body of the animal, attach themselves easily one to another, and to the extreimity of the shell, especially when the more subtil part is evaporated; they then compose all together a little solid body, which is the first lay of the new piece of shell. Other little parts of a like matter to that of the former lay, for the circulating fluids supply this matter in great abundance, escape from the vessels,
by

by the same mechanism, and form a second lay, and thus in a continued series till the new shell hath acquired a *certain thickness*.

I shall now have recourse to experiments for shewing whether I have truly described the manner in which nature acts, or whether all that I have been advancing should be considered only as the sport of imagination.

I began with supposing that the animal grows before its shell, and this is a matter which it is easy to be assured of by observing, with attention, the garden-snails at the time they enlarge their shells; you may perceive very distinctly that the shell is too small to contain them. At that time they fasten themselves to walls, where they continue at rest, and give you an opportunity of observing that a part of their body *juts out* round about the shell. This part, like the rest of their body, is full of a prodigious number of little vessels; the eye *alone* perceives a great number of them, which appears considerably increased by the aid of the microscope.

The pores that I suppose these vessels to be full of are too small to be perceivable by the eyes, but you may be convinced of their existence by their effects, with as much certainty as if you perceived them very distinctly; for this purpose you need only break a piece of the shell of a snail without injuring the
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the animal, which is always easy to be done, because the shell is adherent to it only in a single place, and taking out the piece of shell that is broken, you will see in a little time the skin of the animal covered with a liquor, which could not arrive from the vessels in which it was contained to this outward surface, unless the pores of these vessels permitted it to pass: If to be more assured of the road that this liquor hath taken in order to arrive on the skin of the snail, you take off this liquor, by wiping the skin with a linen-rag, in a few hours *after*, you see the like liquor appear again, which oozes at the same time from all the *bare* part, and by consequence could have passed only through the pores. It is this liquor, or rather the *parts of matter* less proper for motion mixed in this liquor, which serve to make the shell of the snail grow. There will be scarce any room to doubt of it, when it shall be known that *they* repair the loss of the piece of shell which hath been taken away; and this will be clearly seen, if after having striped a snail of a part of his shell, you put him in a place where you can commodiously see him, as in a vessel for example; it is not long before he fastens himself to the sides of the vessel, in the same manner as they fasten themselves to the walls of gardens at the time that their shells grow. You will then see this liquor grow thick and congeal,
or,

or, to express myself more fully, the more subtil parts evaporate, and the grosser remain alone, which form on that part of the body of the animal that is *bare*, a very fine crust; one may often distinguish this crust in 24 hours time; it then resembles very much, by its fineness, those webs that house-spiders make in the angles of walls. It is this crust which forms the first lay of the new shell. You see at the end of a few days this crust grow thicker by means of the different lays that are produced beneath the first; and at length, in the space of 10 or 12 days, most commonly, the new piece of shell that is formed hath pretty near the same thickness as the antient piece of shell that was taken away.

When you would have the new piece of shell *equal* the thickness of the old, it is necessary to have the precaution of putting into the vessel, where the snails are included, a food that is suitable for them. You may give them grass, earth, and paper often wetted with water, they eat indifferently all those sorts of things, which can supply little pieces of matter sufficiently solid for forming the shell.

I must observe moreover, that snails, in the beginning of winter, make themselves a little lid or cover for the mouth of their shell, by which they intirely shut themselves up, *as we mentioned of the Buccinum, Vol. I.*
Plate

plate 3. fig. 4. letter C. Indeed this lid is of a *texture* very different from *that* of the *shell*, but it is solid, and that suffices to shew that there is a great deal of solid matter mixed with their fluids. The difference which is between the *texture* of the shell and *that* of the lid arises without doubt from the difference of the pores, through which their fluids have passed before they form the one or the other.

The manner in which a new piece of shell is formed in the place of that which hath been taken away would alone suffice to prove that shells do not vegetate. For if they grew by vegetation, it could only be in two ways, which it is not possible to reconcile with the preceding experiment: Either the liquors which the animal would furnish for the growth of the shell, and which it could not in this hypothesis communicate to the shell *but* at the *little place* where the shell is connected to the animal, *which* must in this case be considered as the *root* of the shell; either, I say, these liquors would run from *thence* through vessels that would carry them to all parts of the shell; or they would be conveyed only to that extremity that is to be enlarged; now in the one or other of these suppositions it would happen that when you had broken off a piece of the shell, the liquor that flows through that shell would ooze at the opening which hath been made

in it ; and *then* it would be round the circumference of the hole that you would perceive that liquor, which in reality is found only on the body of the animal ; the liquor after coagulating would make a kind of callus, which increasing, by degrees, would at last stop up intirely the hole. It is *thus* that the callus of bones broken is formed by the extravasation of the juice that served before for nourishing them and for their growth ; *thus* likewise when the flesh of any part of the body hath been cut, the neighbouring flesh extends itself, and covers over the part that had been laid *bare* ; lastly, we find the same thing to happen in trees from which a part hath been taken : There is formed a callus from the juice which is extravasated from the tree, and which covers that part of the tree by little and little ; now all is transacted quite *otherwise* in the production of the new piece of shell. Nothing oozes from the shell ; the whole extent of the hole is stopped up *at the same time* by the liquor that issues from the body of the animal ; and that it might not be suspected that this liquor, after it hath been extravasated from the shell in an imperceptible manner, falls by its own weight upon the *body* of the animal, *where* it is collected in a sufficient quantity to compose afterwards the new piece of shell, I am going to mention two experiments which
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will serve equally to dissipate this scruple, and to demonstrate what I have advanced.

I have broke several shells of snails in two different manners. First, I have made in *some* a pretty large hole, between the two extremities of the shell, *that is to say*, between the point of the shell and its mouth; after *which* I introduced by this hole, and fastened underneath the edges of the shell a piece of leather made of the scarf skin of a sheep * (it is of this leather that gloves are made which are called *gands de poule*) this leather was extremely thin, but of a close texture; I fastened it in such manner that it stopped up very exactly the hole I had made. Now it is evident that if the shell was not formed from a liquor that issues *immediately* from the body of the animal, but from a liquor that pervades the shell, it must necessarily follow that a piece of shell would be formed on the external surface of the leather, and it would be impossible for *any* to be formed between the body of the snail and the leather; yet the contrary always happened; that side of the leather which touched the body of the animal was covered with shell, and nothing at all was formed upon the external surface.

The other experiment was not less decisive than what I have been mentioning. Secondly, I broke several shells of snails in
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* In French it is called *Canepin*.

in such manner that I diminished the number of their circumvolutions. I have, for example, reduced the shells of the large garden snails, which commonly make *four spiral turns*, or *four and an half*, to *three turns and an half*, or to *four turns*; thus I have made these shells too little to cover the snail; and I have put them pretty nearly into the same condition they are in, when the growth of the body of the animal occasions them to grow. After having *thus* broken several shells, I have taken, as in the preceding experiment, a piece of leather as large as the circumference of the opening in the shell; I have introduced one of the extremities of this fine leather between the body of the snail and the shell, to the internal surface of which I have fastened this leather; and having laid the other extremity of the leather over the external surface of the shell, I have in like manner fastened it down with glew, so that I have enveloped the whole compass of the opening in the shell with this leather. Now if the shell grew by vegetation, the one or other of these two things must have happened; either the piece of leather, thus glewed to it, would have hindered its growth, or the shell by lengthening would have carried away the leather: But it happened on the contrary that the shell grew, and that the leather remained *where* I had placed it; for the growth of the shell was

was effected in such manner that the thickness of the glove-leather remained between the new piece of shell and the *old, which* by consequence contributed nothing to that formation.

It cannot appear difficult to conceive how the little pieces of solid matter that are mixed in the liquor, may be attached one to another, in order to form a first lay of the new shell; nor how a second lay may be united to the first, a third to the second, and so on through a continued series; or rather this difficulty is not different from what must be acknowledged in explaining the union of the parts of all solid bodies.

The different lays that compose the thickness of the shell become very sensible if you throw shells into the fire and draw them out again after having let them burn a little; the thickness of the shell by that means splits into a great number of different leaves that have separated a little the one from the other; for the fire finds a more easy passage between the different leaves than between the parts that compose each leaf distinctly; this is what commonly happens in bodies formed in strata. All pastry-cooks works, which are commonly called puff-paste, furnish us with a vulgar but very sensible example of it; all the art requisite in pastry-works consists in their being made of different lays of paste and butter laid one upon another; when

when they are baked, they separate into several leaves; the fire opening itself passages more easily, or finding them already open between those different lays, which cannot be *exactly* applied one to another throughout their whole extent.

The divers lays of shells may easily be attached one to another without their being glewed or fastened likewise to the body of the animal which they cover; the moistness of the skin of the animal must probably prevent such a close connection, and if it should happen that they are a little attached *together*, the divers motions of the animal in his shell would be sufficient to sever them.

It remains now to clear up two difficulties which may appear considerable; the first arises naturally from the experiments I have related, and consists in *this*, That the new piece of shell which is formed to stop up the hole that hath been made, is commonly of a whitish colour, and consequently very different from that of the rest of the shell; from whence it might be thought to be of a different texture, and one might conclude from thence, with some degree of probability, that it is not formed in the same manner as the rest of the shell, so that the preceding experiments would decide nothing with regard to their usual growth. In order to give an answer to this difficulty, it is necessary to explain from whence arises the regular variety of

colours in certain shells; the same experiments which will furnish the cause of *that*, will serve to dissipate intirely this difficulty.

The regular variety of colours is particularly remarkable in a *little* species of garden snails; the *ground* of their shell is white, citron, or yellow, or of a colour between these. Different stripes appear drawn on this ground; they wind the spiral of the shell; in some they are black, in others brown, sometimes they are of a reddish colour. The breadth of each of these stripes increases by imperceptible degrees, as they approach towards the mouth of the shell: It even happens sometimes that two of these stripes spread themselves sufficiently to be blended together, and to make but one: Some shells have five or six of these stripes, others have only three or four, even two, or a single one: You may likewise observe divers brown and white stripes upon the *large* garden-snails; but they strike the eye less, and one must look at them with some attention to distinguish them the one from the other: The snails of both sorts that we have mentioned have not *all* these stripes of the same breadth in the same part of the shell.

There appears but one probable way of accounting for the variety of these colours upon the system we have established of the growth of shells by *juxtaposition*: For having
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ing considered the skin of the animal as a kind of sieve that gives passage to the particles which serve to form the shell; it is clear that if one conceives this skin to be differently perforated in different parts, or (which amounts to the same thing) that it is composed of different sieves, some of which let pass *little pieces* of solid matter that are different in figure, or of a different nature from *those* which pass through others, and stop the passage of the *latter*; it will follow that these little pieces of a different figure or nature will be proper to form bodies which will reflect *differently* the light, that is to say, they will form pieces of shell of divers colours.

It is a necessary consequence of the manner in which the shell of a snail grows, that the whole circumference or spiral thread of the shell must be formed by the * *collar* of the snail, because *that* is the part which is the nearest to the head, and by consequence as the animal grows *this collar* must cease to be covered by the ancient shell: It must therefore be always *its* function to enlarge the shell, and we may justly consider *it* as the workman of all the circumvolutions; from whence it will follow that this collar must be composed

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of

* The collar is that mass of flesh which surrounds the neck, as was observed in speaking of the buccinum, vol. I. p. 105.

of *different* sieves or strainers, in order to form a shell of different colours.

Although we should not be able to *see* any thing resembling the different *sieves* or *strainers*, of which I have been speaking, upon the collar of the snail ; yet *they* furnish us with so probable an explanation of the variety of colours in shells, that it would be necessary to *suppose* them there : But luckily they discover themselves, particularly in the little species of snail, which is so remarkable for its distinct stripes. When you have stripped one of these snails of a part of his shell, all the rest of the body appears of a pretty white colour, except the collar, where the white inclines a little towards a yellow, and which besides *that* is marked with a number of black or brown streaks equal to the number of stripes in the shell, and placed in the same direction ; thus the snails which have only a *black stripe* upon their shell, have in like manner only a *black streak* upon their collar ; those which have four stripes upon the shell, have likewise four upon the collar, that are placed immediately beneath those of the shell. The length of these streaks in the collar is different in different snails of the same species. It is impossible to avoid acknowledging the sieves or strainers I have spoke of by observing these streaks, their different colour is an evidence of their difference of texture. In order to leave no doubt

doubt but these streaks perform the function of *ieves*, which differ from *those* of the rest of the collar, and that the rest of the collar, which appears of a different colour from the rest of the skin through the whole body, lets pass likewise particles of a different figure or nature, it is only requisite to observe whether experience agrees with this reasoning; and for that purpose you need only let a snail repair the shell that hath been taken away: For if it happens that the shell which is formed directly opposite these black streaks is black, and that what is formed between them is of a different colour from what is formed upon those streaks and upon the rest of the body, it must appear incontestable that these different places perform the functions which have been attributed to them. Now experience coincides perfectly with the preceding theory: The *shell* which grows upon the collar directly opposite the brown or black streaks is itself black or brown; *that* which is formed between these streaks is white, or of a citron colour; and *that* which appears upon all the rest of the body is white, but of a different whiteness from the collar, when that likewise is white. The same thing happens in the large garden-snails; all the shell that is new formed on their collar is brown, or of a colour resembling that of the

former shell, and the shell which grows again upon the rest of their body is white.

It is proper at present to dissipate another scruple that might arise to those who should attempt the same experiments as I have related. It sometimes happens that the *new* shell, which is formed directly opposite to the collar, in the place of the shell which hath been taken away, is not of the same colour as the *old*; now it should seem from the explication and experiments which I have been relating, that this ought not to happen.

This kind of irregularity will appear less difficult to be reconciled with the reasoning and experiments preceding, if it be considered that the new shell formed opposite to the collar is never of a different *colour* from *that* of the old, *unless* its external surface is extremely *rough* and represents several *furrows*, * whereas the surface of the rest of the shell is very smooth.

The unevenness of that surface of the new shell, is caused by the motions which the snail gives himself when *he* would draw back into his house, before the new shell hath acquired sufficient thickness to support itself without resting upon *him*.

It would be a wrong way of arguing to conclude from what we have said of the formation of the stripes which adorn certain species

* In consequence of *which* the texture must be altered.

species of shells, that the external surface of every individual shell ought to be striped, or of an uniform colour, and that there ought not to be any of these shells whose external surface should be marked with divers stains placed differently, of an irregular figure, and separated one from another by unequal intervals. In answer to this objection I observe, that it is necessary that these strainers should subsist the *same* during the intire formation of the shell, in order to make this shell striped throughout its whole extent; but if it happens on the contrary that these strainers change, that is to say, if the pores, which let pass a liquor proper for forming a shell of a brown colour, become too large or too strait, or change in any other manner their figure, after having strained a certain quantity of this liquor, and those pores, which gave passage to the liquor that forms the white shell, change likewise their configuration, it will happen from thence, that the shell which is formed, will be marked with divers black and white spots, combined in the same irregularity as the change in the strainers themselves.

The fluidity of the liquor which serves to form the shell hath perhaps likewise some share in the irregular distribution of the colours that are seen on some species of shells. For it is easy to conceive that if certain animals let pass, for the formation of the shell, a

C 4

liquor

liquor which is very fluid, and that easily flows from one part to another, there may be formed shells irregularly marked, upon supposition that they have strainers on their collar that let pass different liquors; since in that case it will often happen that the liquor will not remain directly opposite the place where it issued, but *what* was emitted *proper* for making a white shell will go and deposite itself in a place from whence issued the liquor which makes a black shell; as likewise *that* which makes the black shell will flow perhaps on a part from whence issued some other liquor which makes the white shell. Now as this will happen irregularly according to the different positions more or less inclined, in which the animal shall chance to be when the shell is formed, these spots will likewise be placed in an irregular manner.

Although the *collar* of the snail describes the whole circumvolution of the shell, and *that* is sufficient to give it the colours which are *regularly* distributed, yet *it* doth not give it all the thickness which it is capable of having; little pieces of solid matter from the fluids, which escape by the pores of the rest of the skin, increase the thickness; of *this* there can be no doubt if you reduce the *shell* of a large snail to the same number of *turns* as *that* of a small one, the spiral thread appears equal in both, but that of the larger
appears

appears thicker. This increase of the thickness of the shell is particularly remarkable in some species of sea shell-fish that wind in a spiral form; it becomes sometimes so great that the first turns of the shell are at last absolutely stopped up, and that the tail of the inhabiting animal is obliged to lodge in some more distant turns.

The tail of the animal not being *adherent* to the summit of the shell, as some have wrongly thought, it is easy for him to *displace* himself, particularly at the time when the place by which the animal is fastened to the shell is changed (for that place is changed according as the body of the animal forms new spirals): A little snail for example will be fastened by a part of the first turn of its spiral, and when it is become bigger it will be fastened only in the second revolution.

The *growth* of shells being proportioned to *that* of the animals which inhabit them, is performed in a manner almost imperceptible; one may however in the greatest part of shells distinguish very easily their different *degrees* of growth: *They* are marked by divers little eminences parallel to each other, that one might readily take for the fibres of the shell: These eminences surround the circuit of the shell in *such* as are flat or bivalved, and are placed on the broad surface of *those* that wind in a spiral. If
you

you attend a little to the manner in which we have represented shells to grow, you will easily remark that they could not be formed without letting the little eminences we have been speaking of appear ; for each *new* little piece of *shell* must be fastened immediately *underneath that* which precedes it, and which by consequence will be more elevated than the *other*, by the whole thickness that it had at the time when the growing of the animal gave rise to the *latter*, underneath *which* must be placed likewise the piece that is produced afterwards : Thus the shell must naturally be full of a great number of little eminences parallel to each other.

Each shell hath commonly some of these eminences much more distinct than others, and widely distant ; they mark the different times when the shell hath ceased to grow, and are something analogous to the successive shoots of trees. The heat of summer or the cold of winter stopping the growth of the animal that inhabits shells, as is very visible in snails, the extent of the shell cannot be *increased* during those seasons ; but it is not *so* with regard to its thickness, for there continually exhales a liquor from the body of the animal, which serves to thicken the former shell ; so that when the animal begins to grow again in a more favourable season, the new piece of shell that he produces is fastened under a shell much thicker than
when

when he grows by continued imperceptible degrees; and consequently the former boundary must be marked by a greater eminence.

There is moreover *another* thing that renders distinguishable those different places where the shell hath begun to grow after having ceased for some time; *which* is a change of colour that you perceive distinctly on the stripes we mentioned before: The black or brown stripes are in *those places* of a colour much brighter and even sometimes but little different from the colour of the rest of the upper surface of the shell. The cause of this change is not difficult to be found, if it be considered that the *strainers* of the collar, which let pass the liquor proper for forming those black or brown stripes, have their origin at some distance from the extremity of the collar; from whence it follows that the first lay of shell, which is formed *by the extremity* of the collar, must be of a different colour from the stripes: For as the growth of the animal causes the streaks of the collar to *advance* under that first lay of shell, whilst it is yet thin and transparent, the shell cannot prevent the appearance of *black* in those places: But when the animal hath ceased to grow for some time, and the thickness of the shell produced by the extremity of the collar hath, in the mean time, been increased, it will necessarily follow, that those streaks being lodged under a piece
of

of shell much thicker and less transparent, the colour of the stripes will be there less deep, at the time the animal begins to grow again; and thus the colour must be different in those places from that of the rest of the stripe.

It must be observed that the *body* of the snail which succeeds the collar furnishes only a white and shining matter, and in proportion as the *body* advances under that part of the shell which the collar hath framed, it coats with a shining white the whole internal surface of the shell; from thence it is that *this* surface either hath not the same colour as the *external*, or hath no variety of colours.

The figure of certain shells is what may appear at present the most difficult to reconcile with the manner in which we have seen they grow; and this is the second difficulty that I have proposed to myself the task of clearing up. What appears to me the most embarrassing in this affair of adapting our theory of the growth of shells by *juxtaposition* to the figures we find, may be reduced to four heads: First, how is it possible that the *bending* of some shells should change in certain places, or, to express myself more clearly, how can be produced certain shells whose curvature after having continued some time outwardly reflects itself back again? The first figure is the transverse section of one of these sorts of shells. You may observe that after this shell hath turned from

Plate 1st.
fig. 1st.

A in the direction of the letters *CCC*, quite to *EEE*, it turns back to *DDD*. A mere opposition of parts one would think should continue the same bending. Secondly, How are produced the horns that one sees on certain shells? I call *horns* certain eminences which are on some species of shells, and that resemble very much in their figure the horns of some animals. You see these eminences in *figure 1st*, marked by the letters *CCC*. Thirdly, In what manner can be produced the furrows on the external surface of some shells, whilst their internal surface is smooth? Why are these shells thicker in some places throughout their whole length than in others, as may be seen in *figure 2d*? Fourthly, How is that cavity made with which the body of the animal hath no communication, and which runs up the whole height of the shell, as is visible in the common sort of the large garden-snail? With regard to the first difficulty I observe, that it is impossible for the *bending* of the shell to change, without some change in the body of the animal that serves as a mould to it: Now it is easy to imagine probable causes of such a change; as for instance, in the growth of snails it may happen that the *external* fibres of the collar do not grow in the same proportion as the *internal*, by consequence they must draw the collar of the snail towards them, and oblige it to bend itself *back* outwardly.

Plate 1st.
fig. 2d, and
3d.

As

As the difference of length in the fibres of the collar makes us easily comprehend in what manner it reflects itself outwardly; so we shall clearly perceive by attending to the different length of these fibres, how it is possible for the body of several animals to turn in a spiral. For if it be conceived that from the time of the production of these animals the fibres of a certain surface of their bodies are longer than the fibres of the opposite surface, it is clear that the body will bend in such manner, that the surface, whose fibres are the shortest, will form the concave of the curvature, and the surface, whose fibres are the longest, will form the convex, which will be sufficient to make the body of the animal describe a spiral, because it cannot grow without *revolving* thus continually, if the longer and shorter fibres grow in the same proportion. It is true that in the case of which we have been speaking, it would describe only spirals, whose different *turns* would be nearly on the same plain, and few animals have their shell, or body which serves as a mould, so *turned*: the different turns of the spirals of their bodies or of their shells are on *different plains*; but by an additional supposition it will be easily conceived how these *last* spirals are formed. Besides the two surfaces, of which we have supposed that the fibres of the one are longer than the fibres

of the other, we must imagine likewise two other surfaces directly opposite, each of which is comprised between the two preceding, but smaller than they; and must consider these two last surfaces as formed in such sort, that the fibres of the one are *all* longer than the correspondent fibres of the other. Which will oblige the body of the animal to incline to *one side*, and make its body form spirals *described* on different plains.

The reader may, at first view, perhaps consider these suppositions as arbitrary, and therefore conclude this hypothesis to be a mere sport of imagination; but if it be considered that we find in the larger animals different substrata of fibres, running in a variety of directions, of various lengths, and subservient to various motions and determinations; I think we cannot reasonably deny it to be probable that there is the like diversity in these lesser animals, and subservient to the like purposes: In such cases, and under such circumstances, it should always be admitted as a sufficient evidence, that the cause assigned is adequate to the effect.

In answer to the second difficulty, the horns that we find on several species of *shells* are likewise produced by the same mechanism as the rest of the shell. Certain fleshy tubercles that rise up on the body of the fish which inhabit *them*, serve as moulds to these
eminences,

eminences, and according as there are more or fewer of these tubercles formed during the time that the animal grows a whole turn of a spiral, there will be more or less of these horns in the *same turn*; they are *hollow* when the tubercles have remained on the body of the animal during the whole time he hath lived: They are in part *hollow* and in part *solid* when the tubercles have been dissipated *only* in part; and lastly, they are entirely solid when the tubercles have been absolutely *discussed* during the life of the animal.

Thirdly, The furrows that appear on the external surface of shells will not give us more difficulty to explain: It will be sufficient to say that all the extremity of the circumference of the body of the animal is *furrowed*; and in conformity to *this*, we find the shell *striated*, even in its internal surface, to some distance from its extremity. But as the rest of the surface of the body of the animal which inhabits the shell is smooth and soft; in proportion as the animal grows, that part of his body, which is not channelled, comes and corresponds to that part of the shell which is channelled, and what is then supplied by that part of the body *towards* the increase of the shell, serves to fill up the internal *striae*; by which means the shell is *furrowed* only on its external surface, except the first lines alone of the breadth of its internal surface.

Fourthly,

Fourthly, In order to unravel the mystery of the formation of the hole which runs up the whole *flight* of the shell (I call it *flight* from its resemblance to the flight of a staircase) it must be observed, that the superior surface of the collar of the snail is convex, and its inferior surface concave: In consequence of which, whilst the collar of the animal *describes* the different turns of the spiral of the shell, that part of the internal surface which is the nearest to the axis round which it *turns*, forms spirals whose *diameter* is less than *that* of the spirals described by other points of the collar. If we now consider the collar *advancing* and *extending* itself to produce a new piece of shell, and at the same time conceive a certain part of the body advancing and folding itself round the new formed spiral, we may imagine this part of the animal not sufficiently curved, nor sufficiently flexible to *frame* itself perfectly upon that part of the spiral, where the inferior surface of the collar makes an angle with its superior surface; from whence it is clear, that there will remain a little *void* space which must run through all the spirals of the shell.

If the curvature of the spiral *diminishes* sufficiently to let the body of the animal mould itself upon it, *after* the shell hath made a certain number of circumvolutions; it is clear that it must then *no longer* form a hole, and that the hole which is already

formed must be stopped up towards its superior surface; and this is what really happens to snails that are arrived to their last degree of growth.

It would require a long treatise to exhaust this subject, and to explain the several singularities that are to be found in the figures of shells. But I have prescribed to myself narrower bounds, and I do *so* the more willingly, as the Reader may, *himself*, refer the formation of what is extraordinary to some of the heads on which we have been speaking.

Memoirs 1709, pag. 364.

A defence
of the
preceding
theory.

This system was attacked by Mr. Mery, with regard to the shells of muscles, and he proposed two difficulties, to which Mr. de Reaumur gave a reply.

The first and strongest objection is, that muscles are attached to the internal sides of their shells by eight muscular ligaments*, or, to speak more exactly, they are fastened in four places to each of the pieces of their shell; now if shells grew by *juxtaposition*, “ it would necessarily follow, that the muscles
“ detached themselves from *thence*; by re-
“ moving continually and by degrees from
“ the place of their first *fastening* they would
“ be always making a new bed for them-
“ selves; a phenomenon,” adds the learned anatomist, “ that hath not appeared to me
“ in

* Vid. Vol. I. plate 1st. fig. 4. *MM.*

Fig. 1.



Fig. 3.

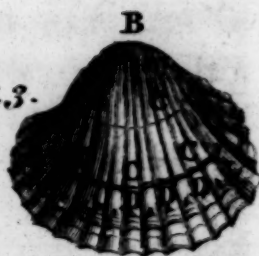
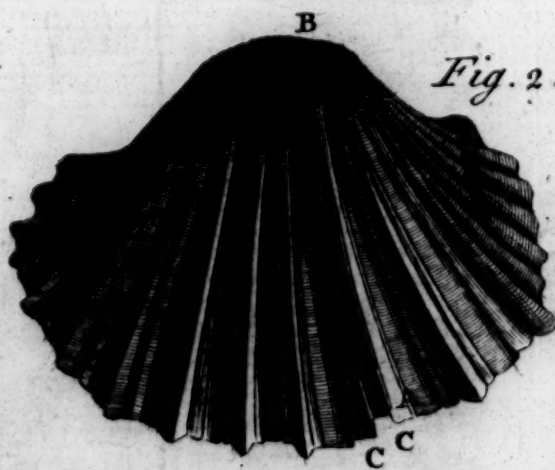
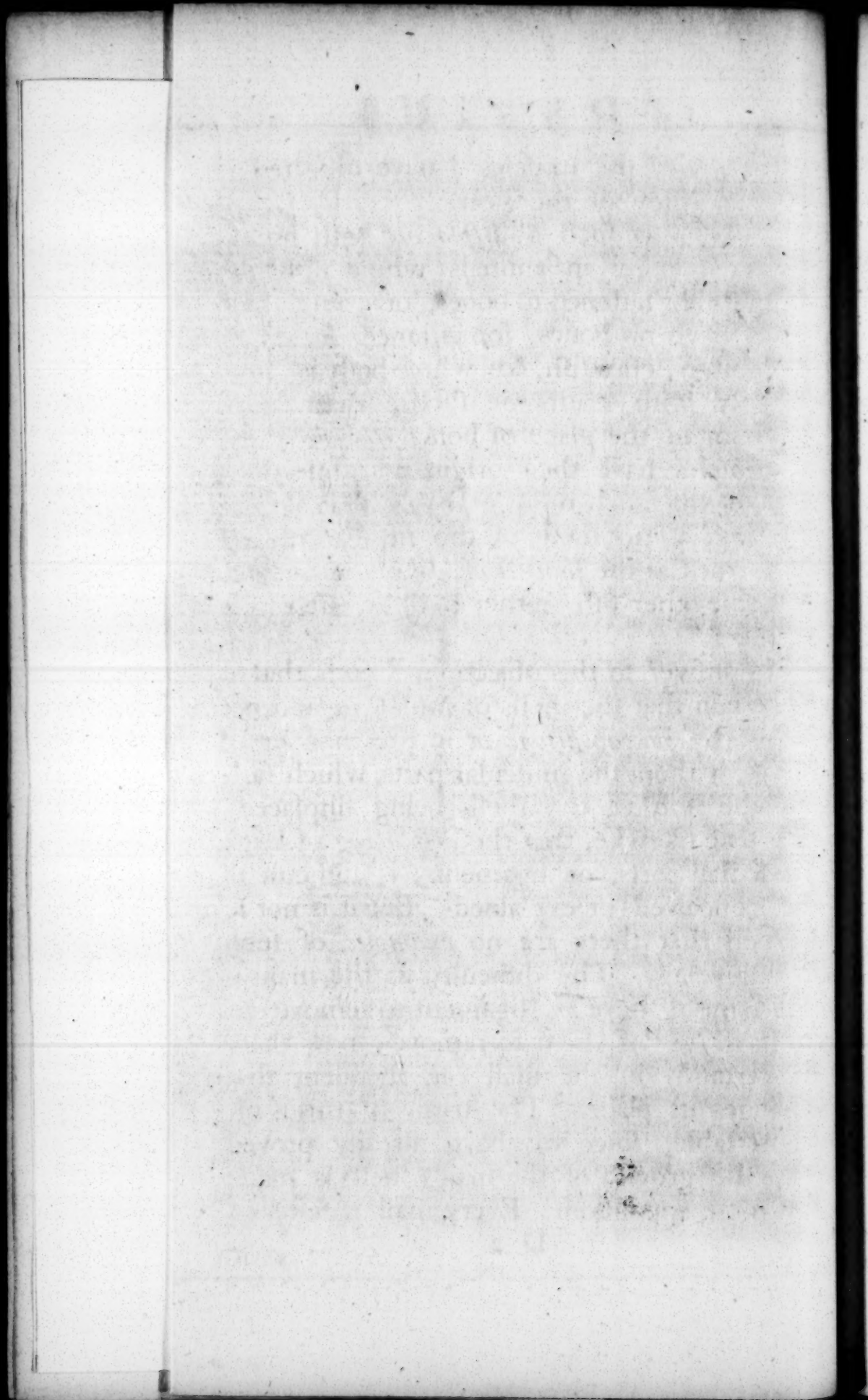


Fig. 2.





“ in any of the muscles I have dissected at
 “ any season of the year.

“ Now as such a *displacing* hath no ex-
 “ ample either in animals whose muscular
 “ parts are fastened to bones, or even in such
 “ as have no bones; for instance, sea-crabs,
 “ lobsters, crawfish, &c. whose body is vested
 “ only with crusts or shells, that are to
 “ them in the place of bones, *where* all the
 “ muscles have their origin and insertion;
 “ is it not much more probable that *all* the
 “ strata of the shells of the muscle-fish are
 “ formed at the *same time*, like the shells of
 “ those other fish, rather than *one* after *ano-*
 “ *ther?* ”

In answer to this objection, I own that it is certain that the shells of muscles could not grow by *juxtaposition*, or in the manner of snails, without the muscular parts, which fasten the fish to the shell, being displaced; it is true likewise, that the *displacing* of large muscular parts, or ligaments, is difficult to be conceived or explained: But it is not so certain that there are no *examples* of such a displacing. The difficulty in the main is the same to have *one* ligament to remove, or to have *four* of them to remove; now there is certainly in the snail *one* ligament that changes its place. The truth of this is incontestable, since we have already proved that the growth of the snail's shell is made by mere apposition. Every snail is fastened

to its shell by a muscular ligament. When the large garden-snail is just hatched, its shell makes only a little more than one turn of a spiral, but makes more than four turns when the animal is arrived at its last boundary of growth: Now in the snail whose shell hath four turns, the muscular ligament is fastened within the shell between the second and third turn; in whatever place you suppose it fastened in the shell of the snail just hatched, it is certain that the ligament must have gone near two turns of a spiral; the only point therefore is to make the ligaments of *muscles* advance like that of the snail, and whatever be the *means* that nature employs for snails, she may employ the *like* for muscles. Mr. Mery adds indeed, that he hath dissected *muscles* in all seasons, and hath never discovered this phenomenon; but that doth not appear to me to strengthen much the proof: Mr. du Verney hath dissected a great number of snails, and in all seasons; I have had occasion to dissect several likewise; and I do not believe that he hath seen any more than I, how the ligament of the snail is displaced; yet it is *displaced notwithstanding that*.

I have therefore already given an example of the displacing of muscular ligaments, and a *case in point* likewise, that is to say, with regard to the growth of shells. It could serve no purpose for Mr. Mery to add, that

that we do not find such a displacing in animals whose muscles are attached to bones, nor even in such as have no bones, for instance *crabs, lobsters, and crawfish, whose body is vested only with crusts or shells that are to them in the place of bones, where all the muscular parts have their origin and insertion.*

All these negative facts would prove nothing against us ; but indeed the example of crabs, lobsters, and crawfish, was unluckily chosen : It is precisely the instance I should have chosen to prove, that muscular parts do displace themselves in certain animals. These, as Mr. Mery hath justly observed, are covered with crusts or shells that are to them in the place of bones, where all the muscles have their origin and insertion. Now we have shewn in the preceding volume, that these animals strip themselves every year of their shells, and in general of every thing they had hard and scaly, of every thing that was to them in the place of bone. *Here, therefore we find a general displacing of all the muscles at least once in a year, since every year they are attached to a shell different from that of the preceding year.*

Let us now examine the second difficulty objected by Mr. Mery. It is visible that the shells of muscles are composed of several strata applied one to another ; which jutting out one beyond another make, on the ex-

ternal surface, very distinct lays. This seems to prove that the *strata* were formed one after another; that *they* mark the different progresses of growth. But what makes the difficulty, is, that Mr. Mery hath observed, *That there do not appear fewer lays on the small shells than on the greatest, and that these lays grow wider in proportion as the body of the muscle increases.*

I shall not insist on its being a vague uncertain difficulty to assert that there are not fewer lays on the small shells than on the great: Perhaps however it might be said, since the one and the other have oftentimes so many, that it is impossible to count them. However I shall grant, that as far as one can judge at the first view, there often appear as many lays on the smallest as on the greatest shells. I shall even add, which will be more than sufficient for Mr. Mery, that when shells are very old, there appear *more* lays on the small than on the great; the little ones have an immense number. But these thin *laminae* are not *proof* against the rubbing of sand, and agitation of water; in vain would you seek for them in old shells; what some had *jutting out* beyond others hath been carried away. On other shells there grows a greenish mouldiness, a kind of moss, that like a membrane, and which would be taken almost for one, extends itself over the shell; this mouldiness

diness conceals a great number of the smallest lays.

It is very true, as hath been asserted by Mr. Mery, that between the lays, which mark the different strata, there are some *wider* in the large shells than in the small: But it is not because the narrow lays have extended themselves; it is that when the fish is arrived to a certain bigness, its growth is much quicker; by this means, as the lays are *so much* as it hath grown in a certain time, there are formed in *those instances* much wider lays: And indeed the widest are most commonly found near the mouth of the shell, unless the friction hath made in other places one single lay out of several. The shells of garden-snails are very proper to give us examples of what I have been mentioning. Their *terms* of growth are very distinct. When they are small, there are often several of these *terms* in a line of length, and when they are large, from one distinct term of growth to another, there are sometimes several lines. Lastly, It often happens that one can find no longer in the *great* shells those boundaries which were marked in them when they were *small*: The frequent rubbings have worn down those inequalities.

Although I think I have given a satisfactory answer to the *two difficulties* proposed, yet I am far from thinking that I have removed *all* which might be on this subject.

I shall even own, that some shells have figures so singular, that it appears scarce possible to imagine how a mere apposition of parts should be able to effect them. But perhaps the unravelling of those difficulties would be very plain and easy, if the figures of the animals that inhabit those shells were known to us; if we knew the changes that happen to them at different ages; in a word, if we could follow the *growth* of their shells with the same care as I have traced *that* of the shells of snails. For example, if we did not know the figure of the fish which lodges in one of the shells called *nautilus*, and what part of it the fish occupies, it would not be possible to imagine how it could be formed by a mere *juxtaposition*. I suppose the reader to know that it is of the class of those shells which are turned in a spiral, that the inside of it is from space to space crossed by *partitions*, having *each* a hole in the middle. How could a fish have built all these partitions, and how, might it be asked, could he lodge himself between them? But this difficulty vanishes almost as soon as one consults the figure of this fish, which Rumphius hath given us in his 17th plate. You there see that the fish is not *himself* turned in a spiral, as his shell is, and as the bodies of other fish with spiral shells are; that this occupies no more than the space which is between the *last* partition and the mouth of the shell;

that probably he is only attached to the summit or origin of the shell by a cord or ligament, that passes through all the partitions. This circumstance being known, it is easy to explain how the nautilus fish builds new partitions. When he is become too big to be at his ease in his ancient niche, and cannot lodge himself on the partition without being too much confined; as the growth of his ligament permits him to change place, he removes from his ancient cloister, and places himself at a suitable distance; there then sweats out from his body a stony juice, such as we have described to ooze from the body of the snail when laid *bare* in any part. This *first* juice that exsudes grows thick, and begins a new partition, *that* which follows afterwards increases the strength and firmness. Snails that have furnished us with so many examples, will *here* supply us again. When in the beginning of winter they shut themselves up in their shell, they make themselves a kind of cloister by means of a lid stopping up the mouth of the shell, as we have mentioned before. If the snail continues a long while included within his shell, so that his body is considerably diminished in size, instead of *one* partition he makes *three* or *four*; you find them quite separate one from another: As *they* mark the different bounds of journeyings that the snail hath successively made *backwards*; so the partitions of the
nautilus

nautilus mark the bounds of progress that this fish hath made successively *forwards*.

Memoirs 1716, pag. 304.

An account of a
floating
meadow.

The abbé de Fontenu, of the Royal Academy of Inscriptions and *Belles Lettres*, hath related to Mr. de Reaumur, that in the parish of Brosigny, near Fougères, upon the frontier of Bretagne, there is a pond that may pass for one of the marvels of that country; it is situated in a valley of about $\frac{3}{4}$ of a league in circumference; the *area* of which it intirely occupies, and receives its waters from abundance of springs, which issue from the hills that surround it. Besides a small number of *floating islands* that you see on its surface, and of which *some* are furnished with trees, it is almost intirely covered by a large meadow closely *adherent* to the causeway, but in no wise fastened to the other borders of the pond: This meadow is intirely supported on the water; when the pond is full, the meadow is on a level with the causeway, and when the waters sink it forms a kind of *glacis*. The reader will see by this description that it must necessarily cover the sluice of the pond, so that when they would let out the water in order to take the fish, it is necessary to make an opening in it; it was on such an occasion that the abbé de Fontenu was able to measure the thickness of this island, which he found to be about six feet; he

he observed at the same time, that this sort of soil was formed of different *strata* intermixed with roots: the *strata* nearest to the surface of the water are the most blackish; *they* appear to be only a very light mould formed by the corruption of roots, weeds, and shrubs that spring up *there* and die every year. It is very certain that it could not have arrived at this thickness, and to cover almost intirely the pond, under a long succession of years. The abbé de Fontenu thinks, that by observing for some years its growth in thickness, one might from thence conclude pretty nearly the time it began to be formed; this sort of crust is of a spongy and moist texture, one may by thrusting in a long stick pierce through its whole thickness, yet the upper-part of it is solid and firm, you may walk on it and imagine yourself to be walking on down, because it is covered over with moss intermixed with other herbs: One might even lead cattle to feed there; but the *borders* of this floating meadow are not so solid as the *middle*, and even *there* you find some places that might be fatal to cattle conveyed thither. It would be natural enough to think that this pond, so covered, should be but little fishy, yet fishing is very plentiful there, and the fish are remarkably large; the abbé de Fontenu hath seen caught there, amongst other large fish, a jack of 24 Pounds, and another of 16.

It

It was reserved for a naturalist so conversant as he in the knowledge of antiquity, to find in one of the singularities of the natural history of France a model in miniature of the famous island of Delos.

History 1745, pag. 17.

We find in classic authors many instances of floating islands, Quædam insulæ, says Pliny, semper fluctuant, sicut in agro Cæcubo, et eodem Reatino, Mutinensi, Statonienfi. And Seneca mentions lib. 3. natur. quæst cap. 25. Ipse ad Cutilias natantem insulam vidi. But the most remarkable instance of a floating island was seen in the year 1707, in the Archipelago, of which an account is given by the academy in their history for the year 1708, pag. 23. But as the same is translated and some further accounts are added in the philosophical transactions, I refer the reader thither. Vid. Jones's abridgement, Vol. II. Part 2d, pag. 196.

These accounts of floating islands will, in some measure, reconcile us to the relations that have been transmitted to us of the prodigious naval fabricks of antiquity, such as that of Ptolemy Philopator, and another of Hiero at Syracuse, built under the direction of Archimedes, wherein were not only contained all apartments for a prince's palace and attendants, but a garden with natural flowers, fruits, fish-ponds, and other usual ornaments of great palaces.

On

On the 19th of October 1742, there was ^{A remarkable agitation of} at the port of Vera Cruz in Mexico, an extraordinary agitation of the sea; it beat down the sea. a part of the walls of the town, and indangered all the little vessels that were stranded between those walls and the sea, where ships had been always considered as perfectly safe. The ships that lay out in the road were obliged to double all their mooring to prevent their being wrecked on the coast. But the most singular circumstance was, that the next day the shore was covered with all sorts of fish lying dead in heaps, and the road likewise full of fish floating on the water; amongst which there were so many uncommon sorts, and unknown to fishermen themselves, that it was impossible to make any numbering of them. They were obliged to employ all the slaves and galley-slaves in burying them in the sand to avoid corruption. Shallops that had been sent out to sea brought intelligence that they had observed the same thing at several leagues from shore, and for the space of between 15 and 20 leagues to the north and south of Vera Cruz. The contagion had communicated itself even to the fish that are commonly found at the bottom of wells in Mexico. During all that time the air had been extremely thick and loaded. The common opinion is, that all these accidents were caused by a noxious vapour that issued from the bottom of the sea;

sea; and what may render this opinion more probable is, that there is at sea, at some distance from the coast, a sulphur-pit that vomits up from the bottom of the water *bituminous* clods, which the winds and waves throw in great abundance on the shore, and which the inhabitants employ to divers uses. A considerable quantity of a poisonous vapour might at the same time cause the excessive motion of the sea, destroy the fish that happened to be within its reach, and penetrate far enough into wells through the earth to stifle the animals that were there. The academy owes this relation to Mr. Daragory captain of a French merchant-man, lately arrived from Mexico, who communicated it to Mr. Hellot.

History 1744, pag. 3.

The preceding account is, I think, a strong confirmation of Mr. Lemery's solution of earthquakes, hurricanes, and water-spouts, which we gave in Vol. I. pag. 15 and 16.

Of bees,
by Mr.
Maraldi.

Of all the insects known by naturalists, the bees are certainly the most admirable. The instinct they have for feeding themselves on flowers, and for gathering from thence the honey and wax; the regularity that reigns in their different functions, their government, their industry, the admirable artifice of their works; in a word, all the properties that
are

are observed in these animals, have drawn the attention of philosophers both ancient and modern.

Amongst the ancients, Aristomachus contemplated them for the space of 58 years, *Aristomachum Solensem*, says Pliny, *duodese-aginta annis nihil aliud egisse*; and Philiscus retired into woods, that he might have more convenient opportunities of observing them; *Philiscum vero Thasium in desertis apes colentem Agrium cognominatum: Qui ambo scripsere de his.*

We owe to Aristotle some very curious and useful observations on this insect, which have been embellished with all the charms of poetry by Virgil. Afterwards these observations were confirmed and enlarged by Pliny, and several other philosophers of antiquity.

Amongst the moderns, prince Frederic Cesi, institutor and president of the Roman academy of sciences, towards the beginning of the last century had wrote, according to the relation of Fabius Columna, a treatise upon bees, which he presented to the pope, Urbain VIII, and gave expectance of to the public, together with the description of the parts of this animal, *drawn* with the assistance of the microscope by Stelluti of the same academy: But it is not known what is become of that work, nor of that which Swammerdam promised several years ago, on the anatomy of this animal.

Swammerdam's *History of Bees* was written in Dutch; he left it, together with his other manuscripts, in legacy to his friend Mr. Thevenot. Mr. Thevenot died before he could perform the duty he owed to the memory of his friend in publishing his manuscripts. Mr. du Verney was so fortunate as to purchase those manuscripts, and by that means instrumental probably in preserving them; but being too much occupied in making new discoveries to publish his own, it is no wonder that he should not perform his promise of publishing those of another. At last the illustrious Boerhaave, from his zeal for natural history, purchased them of Mr. du Verney, and engaged the learned Gaubius to translate them into Latin, and to get them printed in Dutch and Latin, which he hath executed. They make two volumes in folio, the last of which was published in the year 1738. The history of bees, which is contained in this work, answers the idea that Boerhaave had raised of it.

Vid. Reaumur's *Histoire des Insects*, tom. V. pag. 208.

Notwithstanding the observations of so great a number of learned persons, we have not forbore to examine this part of physics, wherein we have been *insensibly* engaged both by the pleasure we took in so curious an enquiry, and by the conveniency we had of a great number of glass hives in the garden
of

of Mr. Caffini, adjoining to the observatory. As several moderns as well as the ancients have treated of the manner of governing these animals with regard to the *utility* which is to be derived from them, we shall not speak on that subject at present; but content ourselves with laying open the treasures of *curiosity* that we have discovered.

You will here see the origin of bees, the different species that are found in *one* swarm, the small number of those that are only for propagation, and the great number of the others that are occupied in labour. We shall explain how these gather on flowers the honey and wax, and how being employed in different functions they mutually assist each other in their labours. We shall give the description of the principal organs of bees, together with an explication of the construction of the cells and combs; a work of ingenious and learned architecture.

The *greatest part* of these observations have been sifted and examined several times, so that they carry with them an intire evidence. With regard to the *others*, that it will be easy to distinguish by the manner in which they are related, we were obliged to be content with conjectures, as they could not be known perfectly on account of the difficulties that attend such enquiries. For *here* nature is not only surrounded with obscurities, as on all other subjects; but she is

moreover *armed* against those that approach her nearly, and would *closely* engage with her.

Of the
different
species of
bees.)

The number of bees in one hive is very different according to the different size of the hives. In the little hives we have counted eight or ten thousand bees, and have found even eighteen thousand in the larger.

In each hive, whether little or great, we have remarked three different sorts of flies. The first is what is properly called *bees*, this species composes almost the whole swarm. These are the bees that go and collect wax on the flowers, that knead it and form from it the combs and cells; these likewise gather the honey and fill with it the combs in summer,

—————*purissima mella*
Stipant, et liquido distendunt nectare cellas,

that it may serve them for nourishment in the winter; these take care to supply their young with food adapted to their age, and to excite a proper heat to bring them to maturity,

Spem gentis adultos
Educunt fœtus.

Lastly, These have the charge of keeping the hive clean, and of removing every thing that may be noxious. All these bees have a
sting,

sting, and there are *some* amongst this species that are somewhat larger than others.

The second sort is what are called *drones*. They are easily distinguished from the rest by their colour, which is a little darker, and by their size; the drones being one *third part* longer and somewhat bigger than the bees. There are hives where one finds but a small number of drones, there are others where you will find a much greater quantity; and there are seasons of the year when we could find none,

Ignavum fucos pecus à præsepibus arcent.

we have likewise sometimes found drones that were not bigger than the common bees. All the drones are destitute of a sting.

Lastly, We have remarked a third sort of bees in the same hive, which are longer still than the drones, but less big in proportion to their length, and of a more lively, reddish colour,

————— *insignis et ore,
Et rutilis clarus squamis.*

We have never found more than three of these bees in an hive, and oftentimes we have found but *one*. This third sort hath a grave and sedate walk, is armed with a sting, and is the mother of all the others. Perhaps it is this sort that hath been called the king.

I must observe that Aristotle had made the same distinctions, and hath given pretty much the same descriptions; although Mr. Maraldi hath not taken the least notice of it.

Κέντρον δὲ αἱ μὲν μέλιτται ἔχουσιν, οἱ δὲ κηφῆνες οὐκ ἔχουσιν. Οἱ δὲ βασιλεῖς ἔχουσι μὲν, ἀλλ' οὐ τύπτουσι. ΚΑΛΟΥΝΤΑΙ ὑπὸ τινῶν ΜΗΤΕΡΕΣ, ὧς ΓΕΝΝΩΝΤΕΣ.—Οἱ δὲ φασιν ὀχεύεσθαι, καὶ εἶναι ἄρρενας μὲν τοὺς κηφῆνας, Θηλείας δὲ τὰς μελίττας.

ARISTOT. Hist. Animal. lib. v. cap. 21.

As Virgil took his account of bees from Aristotle, it is a little surprising that he should not have preferred that opinion I have cited from thence, That what is commonly called the king, is really a female, and the mother of all the bees. This notion is, in effect, more prolific of the marvellous, which is the life and soul of poetry; and that I do not offer a vague conjecture in thinking it more marvellous, I beg leave to refer the reader to Mr. de Fontenelle's Plurality of Worlds, towards the end of the 3d dialogue, where he will find the marvellous on this head, drawn out under a strict observance of truth, to a perfect astonishment. But besides this reason, we know that the very name of king was odious to the Roman people, and therefore, one would think, the poet should have studiously avoided it, where his design was to celebrate the government of bees.*

We

* *Admiranda tibi levium spectacula rerum,
Magnanimosque duces, TOTIUSQUE ORDINE GENTIS
MORES; et studia, et populos, et praelia dicam.*

We have now enumerated the different sorts of bees, and shall proceed to the description of the bee.

One may distinguish three principal parts in the body of the bee, *the head*, which is attached by a kind of neck to the rest of the body; *the middle*, or *breast*; and *the belly*, which is distinguished from the other by a cutting off in the middle.

Descrip-
tion of the
bee.
Vid. Plate
2d. fig.
1st.

In the head we shall content ourselves with observing two kind of talons or jaws, that are in the inferior part, and that open and shut from right to left. This organ serves the bees as hands for taking hold of the wax, for kneading it, for building cells with it, and for polishing them. They make use of these talons likewise for conveying *into* and *out of* the hive every thing that is necessary to them.

Fig. 2d.

At the same extremity of the head, bees have a proboscis or trunk, the origin of which is near the neck. It goes tapering from its root, where it is bigger, quite to its extremity, where it terminates in a point.

This trunk is composed of five branches, two of which are detached from the others quite from their root, the one to the right, the other to the left; the three others do not separate from each other till about the middle of the trunk. The middle branch is cylindrical, of the bigness of a hair on a man's head, and when seen with a micro-

Vid. Plate
2d. fig. 3d.

cope it appears through its whole length distinguished into several rings, each of which is furnished with a great quantity of little hairs, that are longer towards the extremity of the trunk than towards its root. *This part*, which we call more properly the trunk, is one of the principal organs of bees, with which they gather the honey on flowers, as we shall mention afterwards, and with which they take their nourishment.

The other four branches are larger towards their origin, and go tapering on to the very *point*. They are made in the manner of a *gutter*, being concave of that side which is towards the trunk, and convex of the other; they have the consistence of horn; the two branches which are detached the nearest from the root are the largest and inclose the two others. They unite so well together, that they seem to make but one single tube.

Towards the middle of the length of each of these four branches there is a sort of articulation, by means of which they *extend* themselves, or *bend* all at once at the place of articulation. The half of the trunk that is at the extremity bends itself inwards, and lays itself along the other half which is towards the origin; these four branches by bending themselves carry with them the proboscis or middle trunk, which hath no articulation. When these branches are *bent*

inwards, (which is their most common situation) they are contained between the neck and the talons, that we mentioned before; but when they are *lengthened out* (which happens as often as the bee would feed itself or gather honey) the other half advances forth from the head, and besides *that* the middle branch of the five can lengthen itself a little beyond the others, and move itself in all directions in order to *suck* with its extremity the honey from the cups of flowers.*

We have been assured by several observations that the bees gather honey by the proboscis *alone*, and it hath appeared to us that this trunk is a pipe through which the honey can pass. We have even seen the trunk grow bigger and less by turns; we have seen it *swell* the very instant that the bee sucked honey, and this enlarging and lessening was propagated successively from its extremity quite to its root, which made us judge that it was the juice that caused the swelling, as it passed through the capacity of the pipe.

It may be supposed that the proboscis or middle trunk, is in the nature of a tongue, and that the branches perform the office of a bill; the tongue, after having sipped the honey from a flower, conveys it by means of

E 4

the

* Mr. de Reaumur conjectures that the tip of the proboscis rather performs the office of a tongue in lapping; and that the liquor is conveyed along the pipe, by the alternate dilatation and contraction of muscles: If so, what a delicate apparatus must there be of invisible muscles!

the branches, quite to their roots, where it is received into the body of the bee in a kind of reservoir. These are the principal parts of the head and their uses, as far as their smallness will permit us to know them.

Vid. Plate
2d. fig.
1st.

The middle or breast of the bee is of a figure approaching to an oblong spheroid, to which are fastened two wings, one to the right, the other to the left. Each of these wings is accompanied by another which is as it were adherent to it, but smaller than the other that is nearest to the head; it is with these four wings that they make sounds or hummings, in order to give notice one to another.

Fit sonitus, mussantque oras et limina circum.

It is likewise in this part of the body, towards the under side, that are situated six legs, three on the right, and three on the left. Two of these legs are on the fore-part, and very near the head; they are the least of the six: The four others are attached on the back-part towards the belly, very near the one to the other: The two middlemost are a little longer than the first, and shorter than the hindmost. All these legs are distinguished by several joints, of which there are *three* greater than the rest; besides those *three* joints that are towards the middle of the leg, there are likewise *others* towards

the root and towards the extremity of each leg. The middlemost joint of the two hinder legs is much larger than the others, and there is on the external side a little concavity, in the shape of a marrow-spoon, which is surrounded by a great number of small hairs; it is in this *hollow* that the bees collect, by little and little, the particles of wax, which they gather on flowers. It must be observed, that the *legs* of drones who do not gather wax, and *those* of the king, or more properly the queen of the bees, have not this groove.

The extremities of the six feet terminate in two hooks, *with their points opposite to each other*, with which the bees fasten themselves in clusters to the sides of the hive and form divers figures, sometimes of a cone, sometimes of a garland, &c. From the middle of these two hooks, there comes out a little thin appendix that folds itself double through its whole breadth; its usual state is to be folded, and when it is extended it appears twice as big; the bees make use of this part for fastening themselves, and for walking on polished bodies, such as glass: I believe likewise that they make use of this part as of hands, to take the little particles of wax upon the flowers, and to transmit them to the two hinder feet; for it is not probable that the *books* can serve for that purpose.

The

Vid. Plate
2d. fig.
4th.

Vid. Plate
2d. fig.
5th.

The last part of the bee which is the belly is distinguished by six rings. In the inside we shall remark two *parts*, the one is a little bladder or reservoir in which is collected the honey that the bees sip in the cups of flowers, after it hath passed through the proboscis and through a very narrow pipe that traverses the head and breast of the bee. This bladder, when it is full, is of the size of a small pea, it is transparent in such manner that you see through it the colour of the honey it contains.

The other remarkable part is the sting, which is situated at the extremity of the belly of the bee, and which is drawn *in* and darted *out* with great quickness, by means of certain muscles placed very near the sting; the length of the sting is about two lines; it is a little bigger towards its root than near its extremity, which terminates in a point. It hath the consistence of horn, is hollow within in the manner of a tube, wherein passes the venomous liquor, which being included in a bladder placed in the belly and at a little distance from the root of the sting, discharges itself near the point, and insinuates itself into the wound, the very instant that the bee pierces the skin.

The bee leaves almost always the sting in the wound, and the sting draws after it the bladder, and sometimes a part of the guts of
the

Fig. 1.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



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the insect*. If care be taken to draw out immediately the sting from the wound, there will be only a little tumour, because you do not give time sufficient for the venom, that issues from the bladder, to enter into the wound; but if you are not diligent to draw the sting out, all the venom discharges itself from the bladder, and enters in a little time into the wound, which causes a great swelling with a pain that lasts sometimes several days.

One of the first occupations of the bees, ^{Of the cells.} after the new swarm is lodged in an hive, is to form cells. They apply themselves to this work with so much diligence, that we have seen them make, in one day, a comb which was a foot long and six inches wide, that, according to the usual size of cells, might contain near four thousand.

They begin their work by fastening it to what they find most solid in the upper part of the hive, and they continue it from above downwards, and from one side to the other. In order to attach it more firmly, they employ sometimes a wax which is a sort of glue.

It is not easy to know, in particular, the manner in which they employ themselves at this work, on account of the number of
bees

* *Illis ira modum supra est, læsæque venenum
Morsibus inspirant, et spicula cæca relinquunt
Adfixæ venis, animasque in vulnere ponunt.*

bees that are in motion, where there appears to the sight scarce any thing but confusion. We have been able however, to observe the following particulars. You see bees bearing each in their talons a little piece of wax, and running to the places where they are at work upon the combs: When they are arrived thither, they fasten their wax to the work by means of the same talons, which they apply sometimes to the right and sometimes to the left. Each bee is employed on this work but for a short time, after which she goes away; but there is so great a number that succeed one after another, and with such expedition, that the comb continues increasing very perceptibly. In proportion as *some* bees work in the construction of the cells, there are *others* that pass and repass several times, beating with their wings and with the hinder part of their body upon the work itself, which seems to serve no other purpose than to make it more solid and firm.

The order they observe in the construction of the cell is this. They begin with laying the basis which is composed of three rhombus's or lozenges. They build first one of these rhombus's, and draw two *faces* on two of the sides of the rhombus; they add a second rhombus to the first in a certain *inclination*, and draw two new *faces* on the two sides of that rhombus; lastly, they add
a third

a third rhombus to the two first, and raise on the two external sides of this rhombus two other faces, which with the four others form the cell, that by this disposition of the basis becomes necessarily of an hexagonal figure.

Whilst a part of the bees is occupied in the construction of the *cells*, there are others of them employed in *perfecting those* that are newly modelled; which they *do* by means of their talons, with which they *round* the angles in an exquisite manner, and *finish* the sides and bases with so great a delicacy, that three or four of these sides placed one upon another, have not more thickness than a leaf of common paper; and because on account of this delicacy the entrance of the cell, which is just adapted to the size of the bee, would be apt to break; therefore to prevent this rupture, they *fortify* the entrance of each cell with a border of wax*.

We have said that the bees which build cells are commonly employed in it but a *little while* at a time; but it is not *so* with regard to those that polish them, for they work a long while and with great expedition, and they never intermit their labour unless it be to carry out of the cell the particles of *wax* which they take off in polishing. In order that *this* matter should not be lost, there

* ——— *grandævis oppida curæ,
Et munire favos, et dædala fingere testæ.*

there are other bees attentive to receive *it* from those that polish, or who go to take *it* up in the cell itself, from whence the polisher retires for a moment, and carry *it* to be employed in some other place.

There are other bees destined to aid those that polish, for one often sees *attendants* on them to give them either honey, or some other liquor necessary for their works, or for their own nourishment.

Each comb hath two rows of cells opposite to each other, which have their *common* bases. The thickness of each comb is a little less than an inch; thus the depth of each cell will be about five lines. We have found in divers combs, that were a foot long, between 65 and 66 ranges of cells; there will therefore be a little less than two lines for the breadth of each, which is a little more than a third of its length.

Almost all the combs are built with cells of this size; except a small number of others in some places of the hive which have larger. The breadth of these cells is something more than three lines, and the length about six lines. These great cells are made for lodging *worms* that become drones, as we shall mention afterwards.

One finds likewise, in divers parts of the hive, three or four cells bigger than the others, and made in a different manner. They have the figure of a spheroid, they are open

open in the inferior part, and attached to the extremities of the combs. We could not discover to what purpose these cells are designed, but we suppose them the cradle or the abode of the kings.

The bases of all the combs are placed at such distance one from another, that when the cells are finished, there remains between one comb and another, only a sufficient space for the passage of two bees a-breast. These combs are not continued from top to bottom, but they are often interrupted; and besides *that*, they have openings from space to space, which give a more easy and a shorter communication from one to another.

After having explained in what manner the bees build the cells, it is necessary to consider more particularly their construction.

Every basis of a cell is formed by three rhombus's almost always equal and alike, which according to the mensurations we have taken, have the two *obtuse* angles each of 110 degrees, and consequently the two *acute* each of 70 degrees.

N. B. *Mr. Maraldi had found by mensuration, that the obtuse angles of the rhombus's were 110° nearly; upon which he observed, that if the three obtuse angles which formed the solid angels above-mentioned, were supposed equal to each other, they must each be of 109° 28'; from whence it has been inferred, that this last was really the true and just measure*

sure of them. Mr. de Reaumur desired Mr. Kænig, a worthy pupil of the famous Bernouillis, to seek what should be the quantity given to this angle, in order to employ the least wax possible in a cell of the same capacity; and that gentleman found, by a higher geometry than was known to the ancients, by the method of infinitesimals, that the angle in question ought, in this case, to be of $109^{\circ} 26'$. This observation by the famous mathematician Mr. Kænig, was afterwards verified by the excellent Mr. Mac Laurin, who hath shewn the advantages arising from this construction of the cells. Mr. Mac Laurin very justly observes, that the bees do truly construct their cells of the best figure, and that not only nearly, but with exactness; and that their proceeding could not have been more perfect from the greatest knowledge in geometry. How they arrive at this, and how the wonderful instinct in animals is to be accounted for, is a question of an higher nature*; this is surely a remarkable example of this instinct, which has suggested a problem that had been overlooked by mathematicians, though they have treated largely on the maxima and minima; and such an one, as has been thought to exceed the compass of the common geometry.

Vid. Philosophical Transactions, N°. 471. and Martyn's Abridgement, Vol. IX. pag. 2.

The

* His quidam signis atque hæc exempla secuti,
Esse apibus partem divinæ mentis, et haustus
Ætherios dixere. Deum namque ire per omnes
Terrasque, tractusque maris, cælumque profundum.

The bee that is named the king is in reality the mother of all the others. She is so prolific, that, as far as one can judge, she may produce in one year eight or ten thousand young ones; for she is commonly a part of the year *singly* in a hive, and at the end of summer the hive is as full of bees as in the beginning of the spring, yet there goes out every year a *swarm*, and sometimes *two* or *three* of ten or twelve thousand *each*; it follows therefore that this royal bee must produce a part of those different swarms: I say, *a part*, because it is possible that the new king, who goes out with the fresh swarm, may produce likewise a part of them before the migration.

The royal bee is most commonly concealed in the most secret part of her palace, and is never visible but when she would lay her young in the combs that are exposed to light.

It was on those rare occasions that we perceived her; indeed she is not even *then* always visible, for most commonly there is at those times a great number of bees, that fastening themselves one to another, hang down in the form of a veil from the top to the bottom of the hive, which hinders your sight, and they do not retire till the bee hath laid her young.

Whenever she hath appeared to us *unveiled*, she was always attended by ten or

twelve of the stoutest bees amongst the common sort, that make a kind of retinue, and follow her wherever she goes with a sedate and grave tread,

*Præterea regem non sic Ægyptus, et ingens
Lydia, nec populi Parthorum, aut Medus
Hyaspes*

Observant —

*Illum admirantur; et omnes
Circumstant fremitu denso, stipantq; fre-
quentes.*

Before she lays her young, she puts, for a moment, her head into the cell where she designs to lay them; if she finds this cell empty, and there is not in it, either honey, wax, or any embrio, she turns herself immediately to introduce the posterior part of her body into the same cell, and sinks into it till she touches the bottom. At the same time the bees, her attendants, who are disposed in a circle round her, having all their heads turned towards her's, pay a sort of homage with their proboscis and feet, caress her, and give her all kinds of entertainment, which lasts however but a very *little while*; after *that* the bee leaves the cell, and you may discern a little white egg, very small, about half a line long, or three quarters of a line at the most, yet four or five times longer than it is big, a little more

I

pointed

pointed at one extremity than at the other, and planted by its *least* extremity on the basis in the solid angle of the cell. This egg is formed of a membrane, thin, white, smooth, and full of a whitish liquor.

Immediately after the pregnant bee hath laid an egg in one cell, she goes with all the same circumstances, and escorted by the same number of bees, to lay another egg in a neighbouring cell; and we have seen her lay in this manner eight or ten in different cells successively one after another. After having finished her *delivery* she withdraws, attended by the same bees, into the secret apartments of the hive; where she is lost out of sight.

The egg which remains on the basis of the cell continues four days in that state without changing figure or situation; but after the four days you see it changed in the manner of the caterpillar, divided into several rings, laid and applied on the same basis, and twisted round, so that the two extremities touch each other. It is *then* surrounded by a little liquor, which the bees take care at the end of the four days to put in the solid angle of the basis. We could never discover the nature of this liquor on account of its small quantity; which hath left us in doubt, whether it might be honey that the bees carry thither for the nourishment of the embryo, or rather some matter

proper to fecundate the sperm; for it appeared to us more whitish, less liquid, and less transparent than honey.

Of whatever nature this first liquor may be with which the little worm is surrounded, it is certain that afterwards the bees bring it honey for nourishment. In proportion as it grows they supply it with a greater quantity of food, quite to the eighth day from its birth, when it is increased in such manner that it occupies the whole breadth of the cell and a part of its length. After that, the care of the bees for the young ones ceases, for they stop up with wax all the cells, where these worms continue still shut up for twelve days. During that time, there happen to the embryos inclosed divers changes; which we have discovered by opening these cells on different days from the time they had been stopped. At first the worms change their situation, and from being twisted round, as they were before on the basis of the cell, they extend themselves along its whole length, and place themselves with the head turned towards the mouth of the cell; the head of the worm begins to shew itself a little, and you see a small *extension*, which is, in my opinion, the beginning of the proboscis. You see likewise upon the origin of the head a black point, and at a little distance from this point a black streak upon the back, which doth not reach quite

to the extremity of the worm; the first lineaments of the feet likewise appear, but very small.

After the head is formed, and the proboscis lengthened, all the other parts display themselves successively; so that the whole worm is changed into an *aurelia* or nymph, which is the *fly* almost perfect, except that it is yet white and soft, and that it hath not that kind of crust with which it is covered afterwards.

By this transformation the worm strips himself of a white and very fine pellicle, which is so perfectly attached to the internal sides of the cells, that it takes even the turns and bendings of the angles as well of the basis as of the sides, and appears to form but one body with them.

The bee being stripped of this pellicle, and all the parts unfolded by degrees and changed through successive colours from a yellow to a black, arrives at perfection by the *twentieth* day from the birth. From *thence* she endeavours to issue from the cell, and makes the opening herself by cutting round with her jaws or talons the *cover* that stopped up the mouth of the cell, *which* the bees had made to inclose her. The new bee when she first quits the cell, appears a little drowsy, but she soon assumes the natural agility, for we have seen her the same day issue from the cell and return from the fields

loaded with wax like the rest. You may distinguish these young bees by the colour, which is a little more blackish and by the hairs, which are somewhat whiter.

As soon as the young bee hath sallied from the cell, there come immediately *two* of the old bees, *one* draws out the cover, kneads, and employs the wax elsewhere of which it was composed; the *other* labours to repair the breach; for the cell having been disordered by the new-flown bee, an old one restores its symmetry, gives it its former hexagonal figure, fortifies it with the usual border, and cleanses it by taking away the little pellicles of the young bee which have remained there. These pellicles of bees which are attached to the cells occasion them to change colour, and it is on this account that you find in a hive, combs of different colour, those in which there hath been nothing but honey being of a bright yellow, whilst those, from whence bees have issued, are of a dusky yellow. We have taken away, sometimes, from a cell, which had been the cradle of several bees, eight of these pellicles placed one upon another.

The cells being restored to their former perfection, the bees sometimes lay new eggs in them on the same day; sometimes they put honey in them first; we have seen bees lay their young in the same cells five different times in a little more than three months.

The

P H Y S I C S.

71

The bees gather two very different sorts of wax. The first, which is brown and gluish, serves them for stopping up all the vent-holes of the hive, and sometimes as a support for fastening the combs to the hive.

Of the manner in which bees collect wax.

—————*tenuia cerâ*
Spiramenta linunt, fucoque et floribus oras
Explent, collectumque hæc ipsa ad munera
gluten,
Et visco et Phrygiæ servant pice lentius
Idæ.

The second sort is the common wax which they employ in the construction of the cells.

—————*pars intra septa domorum,*
Narcissi lacrimam, et lectum de cortice gluten,
Prima favis ponunt fundamina, deinde te-
naces
Suspendunt ceras.

The bees gather the common wax upon the leaves of a great number of trees and plants, and on the greatest part of flowers that have *stamina*.

—————*pascuntur et arbuta passim,*
Et glaucas salices, casiamque, crocumque
rubentem,
Et pinguem tiliam, et ferrugineos hyacinthos,
Purpureosque metunt flores—hinc arte re-
centes
Excudunt ceras.

They often load themselves intirely from *one flower*; but they work with such a prodigious quickness, that whatever attention you give in observing them, the eye can scarce keep pace, and ascertain the manner in which they act. It is certain, however, that they sometimes gather up the wax with the hairs that garnish their bodies, by rolling themselves on the flower; for you see them return from the fields with their hairs loaded with little particles of wax resembling dust: But this only happens when the mornings are moist, the moisture that is then on the flowers preventing perhaps the particles from being easily compacted into that part of the body where they are accustomed to place them. When they are arrived at the *bive*, the heat *there* causing the moisture to evaporate, they easily brush off with their feet the wax upon their hairs, and collect it together.

They commonly gather up the particles of wax with their talons and the two fore-feet, from these they convey the *particles* to the middle feet, which afterwards transmit *them* to the middle joint of the two hinder feet, where you find the little mass collected of the bigness and shape of two small lentils.

Crura thymo plenæ :

That

That joint is much larger than the others, as we have mentioned before, and hath a little concavity in the shape of a spoon, which is well adapted for containing the mass; moreover this concavity is surrounded by little hairs, which serve, as I may say, in the nature of fingers for *holding* the wax in that place, in order that it may not fall out when the bees return to the hive.

Besides these means which nature hath furnished them, they take likewise other precautions not to lose the fruit of their labour. In proportion as the bees transmit the particles of wax to their hinder legs, they *compress* these particles together; this they *do* by means of the two middle feet, which they push backward and apply several times and in different directions upon the wax. They take this care principally, when loaded with a sufficient quantity of wax, they are ready to fly and return to the hive: If the flowers on which they rest, have not sufficient consistence, or are agitated by the wind, they seek some more *stable* place and that is more proper to resist the little compressions they make on the wax.

The bees being arrived at the hive unload themselves of the common wax in two different manners. Supporting themselves on their two fore-feet, they make several motions of the wings and body, sometimes to the right, sometimes to the left; and,

as if this motion, and the buzz that the wings make in consequence of it, was to give notice to their companions within, there come three or four that take each a small portion of wax with their talons,

Aut onera accipiunt venientum.

To these first succeed several others that take every one their share, till there remains no more wax on the leg of the bee: After which she returns to the fields to make a new collection.

It is likewise in this manner that they unload themselves of the other sort of wax which is, to speak more properly, a kind of glue,

Et visco et Phrygiæ—pice lentius Idæ,

which sticks so fast to the leg of the bee that is loaded with it, that it is necessary for the bees that unload her, and for herself, to exert their whole force, and to fasten themselves whilst they pull in order to draw it off.

But when there is in the hive a great number of cells where they may deposit the common wax, they practise a more expeditious method, and that requires no assistance. The bee which is loaded, goes in quest of a cell in which there is neither
honey

honey nor any worm ; having found one, she fastens herself by the two fore-feet upon its superior border, after *that* she bends the body a little forward in order to put her two hinder feet into the cell ; in this situation she thrusts back her middle feet, one on *this side*, another on *that* ; and sweeping with them from top to bottom along the two hinder legs, where the two lenticular masses are collected, she detaches them by this means and deposits them in the cell.

There are some bees that content themselves with *leaving* the wax in that part of the cell, where it chances to fall when they detach it from their feet, without giving themselves the trouble to *dispose* it ; but the greatest part, after having unloaded themselves, enter into the cell and range very neatly at the extremity of it the two little bodies of wax by side each other : That done, the bee retires.

Almost immediately there comes another ; there are even *some waiting* till the first bee is gone out, in order to enter and perform their respective offices. If the two pieces of wax are not ranged as we have mentioned, *they* carry them to the farther extremity of the cell, and dilute them, with their two jaws, for the space of half a quarter of an hour ; so that those two little bodies are reduced to a kind of paste that takes the figure of the cell as in a mould ; which gives one
reason

reason to think, that the bee, in diluting the wax, mixes with it some liquor, either honey, or perhaps only a mere moisture that naturally oozes at the place where they are accustomed to *store* their honey.

Several other bees come to unload themselves in the same manner in the same cell, and as fast as one unloads herself of the wax, there comes another immediately to perform the same office of diluting it, till the cell is almost full of that sort of wax, which is piled up sometimes in *stories* of divers colours, white, yellow, red, and brown, according to the flowers or leaves on which the wax hath been gathered by different bees.

One finds in several places of the hive a great number of cells full of this wax, which are as so many magazines, to which they have recourse on occasion, because as they have need of it a great part of the year at certain times, in order to *cover* the cells, where their young are inclosed, and to stop up *those* that are full of honey, it is necessary they should have stores and provisions of it.

The *wax* which is found in the cells is not however so perfect as *that* of which the combs are formed; for although the former be diluted with moisture, it crumbles into dust when pressed between the fingers, whereas the other wax is a kind of compacted
 4 paste;

aste; it is necessary therefore that the bees before they employ it in the construction of the combs, should give this wax some sort of preparation. What still further evinces this is, that the wax included in the cells, which is often of different colours, is always white immediately after the combs are built.

The bees gather honey on the flowers whose calyx or cup is not much deeper than the length of their proboscis; but there is so little honey in each flower, that they go over a great number, Of the manner in which bees gather honey.

Floribus insidunt variis,

before they can collect a sufficient quantity to fill their little bladder, which is the reservoir where it is gathered together, as we mentioned in the beginning. The very instant a bee settles on a flower, she extends her proboscis quite to the bottom of the cup, from whence she sucks the honey. When their bladders are full, the bees return to the hive, and carry the honey into a cell, disgorging it by that part of the head which is between the two jaws or talons, that they lengthen out more than usual, and which they seldom keep open. They deposite the honey with stirring the head sometimes on one side sometimes on the other; and when there happens to be a drop not well ranged, they extend their proboscis to gather it up again,

again, and place it in the same order as the rest. As the *honey* that a single bee can carry at once is but a small part of *that* which a cell can contain, it requires the honey of a great number of bees to fill each cell.

When the cells are full of honey, if the bees would hoard it up for winter, they stop up the cells by making a thin cover of wax; but those cells, where the honey is designed to serve for daily food, remain open, and at the disposal of the whole swarm*. The honey which they reserve the *last* for their food, is always put in the most inaccessible place, that is to say, in the upper part of the hive, if it hath no *external cover* that can be taken off; but if it hath *one*, they leave in the upper part *empty* combs, and place the honey towards the middle of the hive.

Divers
particula-
rities of
bees.

Besides what we have mentioned hitherto with regard to bees, nature hath furnished them with other talents, which we have thought it our duty to remark. They love cleanliness and do every thing in their power to preserve it.† The glue that they gather serves them for covering with a kind of mastic the glass-windows round the hive, and the hive itself round the pedestal, insomuch that by this means they hinder the entrance of the least insects.

— nam

* ——— in medium quæsitæ reponunt.

† Neu propius tectis taxum sine, neve rubentes
Ure foco caneros, altæ neu crede paludi,
Aut ubi odor cæni gravis;

—nam sæpe favos ignotus adedit
Stellio, et lucifugis congesta cubilia blattis !
Aut asper crabro imparibus se immiscuit
armis ;
Aut dirum tineæ genus, aut invisa Minervæ
Laxos in foribus suspendit aranea casses.

There are some bees that remain centinels at the mouth of the hive,

Sunt quibus ad portas cecidit custodia sorti,

in order to oppose the insects that would pass by that entrance, and when *one* bee is not sufficiently strong to guard it, several others come to her succour.

We should be tedious if we were to relate *all* that we have seen done remarkable on such occasions. It will be sufficient to mention, that a snail which had entered into the hive, notwithstanding the resistance of several bees, after having been killed by their stings, was embalmed all over with that mastic we have mentioned, as if with design to prevent either the stink that the putrefied flesh would occasion in the hive, or to avoid the worms which that corruption might produce.

Nature hath indued the bees with an exquisite smell, for they scent at a great distance the honey and wax.

They

They have divers manners of giving each other *pleasure*, of which they appear extremely *sensible*. They are likewise subject to fight and kill each other, not only in single combat, but *general battles*; yet *this* never happens in common, unless in autumn the harvest of honey is not sufficient for the support of the whole swarm during the winter.

Virgil, instead of this physical cause of their civil wars and destruction, assigns a political reason ;

——nam sæpe duobus
Regibus incessit magno discordia motu.

I cannot find that Virgil hath copied this reason from any of the ancient naturalists preceding his time, it seems therefore to be a mere fiction of the poet, with a view perhaps to throw an odium on regal powers; as Horace hath likewise delicately flattered the Roman disdain of kings with

——Delirant reges, plectuntur Achivi.

It seems as if the bees had some foresight of *fine* and *bad* weather, for not only they do not go out when there is an appearance of wet weather,

*Inque vicem speculantur aquas, et nubila
cæli ;*

but

But when a storm seems to threaten at the time they are in the fields, they escape it by quitting their labour, and arriving at the hive almost all at once, and with great precipitation.

Nec vero a stabulis pluviam impendente recedunt

Longius, aut credunt cælo, adventantibus euris;

*Sed circum tutæ sub mænibus urbis aquantur,
Excursusque breves tentant, et sæpe lapillos,
Ut cymbæ instabiles fluctu jactante suburram,
Tollunt; his sese per inania nubila librant.*

Nothing agrees better with bees than heat; the greater it is, the more animated they are, the more lively for labour: cold on the contrary is so injurious to them, that however animated they were within the hive, when they go out of it in the winter they are numbed and struck almost immediately motionless.

—————*contracto frigore pigræ.*

If you bring them near the fire, the warmth they receive from thence reanimates their former vigor.

In order to defend themselves from cold during winter, they croud about the middle of the hive as near to each other as they can be in the space that is between two combs. There they stir themselves from time to time,

without changing place, and this motion excites a warmth that protects them from the external cold: The heat is so great by this agitation, that it is communicated to the glass windows of the hive, where it is very sensible to the hand that is applied.

It is probable that they succeed one another by turns in labouring, because they work night and day in the hive, and there is a part of the bees that repose themselves even in the day time.

Virgil on the contrary, following Aristotle, says,

Omnibus una quies operum, labor omnibus unus.

Their repose however doth not cease to contribute to the public good; for their presence in the hive causes a warmth, which serves to breed the young that are inclosed in the cells, as we have discovered by the following experiment.

We have sometimes broke off large pieces of combs, where there were little worms in the cells, and left them at the bottom of the hive. A great number of bees have gone and placed themselves on those broken combs, and continued *there* till all the brood have sallied forth in the form of bees. This experiment shews moreover the care that the common bees take of the young ones.

Sola

*Solæ communes gnatos, consortia tecta
Urbis habent.*

We have discovered that they have divers manners and different motions by which they *understand* one another; as, for example, when a bee that is at work on the combs demands honey of one that is just arrived; she that wants the honey extends her proboscis and puts it between the talons of her that is to give the honey; in proportion as the *latter disgorges* it, the other receives it with the proboscis, without spilling a drop.

They *understand* one another likewise, *when*, by a motion of their wings, they ask, as it were, to be unloaded of the wax, which they have gathered in the country; likewise *when* in the morning they rouse each other to go out to work :

Mane ruunt portis, nusquam mora.

Lastly, *When* several bees would quit a place, if one makes a motion with her wings which causes a little buzz, all the others following the example make the same motion, and retire. I imagine, that it is in this manner they give notice in the hive, *when* they are preparing to fall forth for making a new swarm.

The drones are commonly a third *bigger* Of the and *longer* than the bees; their head is round- ^{drones.} er and more full of hairs. It is certain they

have no sting, and their *internal* parts are very different from *those* of the common bees.

One rarely sees them go out of the hive, and whenever they do go out, it is not till two or three a clock in the afternoon, and never but in fine weather. They do not return loaded with wax; but we have found their bladder or reservoir *full* of honey as in the common bees, whether it be that they have collected it in the fields, or that they took it in the hive before they went out, which latter we are the most inclined to believe, because we have never seen them settle upon flowers, and after they have returned into the hive, we have never seen them lay the honey in the cells;

Immunisque sedens aliena ad pabula fucus.

We are even apt to think that they have not the organs for *disgorging* it, as the other bees *do*; for in the other bees you cannot compress, ever so little, that part of the body which is opposite the bladder when it is full of honey, but you see the honey *issue* immediately at that part of the head by which they are accustomed to discharge it into the cell; whereas it is not *so* with regard to drones, although after having opened them, their bladder hath been found quite full of honey.

In

In some hives the drones are few in number, in others they are extremely numerous. During a part of the summer they are *dispersed* in the hive. Afterwards in proportion as their number increases, they *assemble together* in companies, in different parts of the hive, where they continue cantoned and almost intirely motionless.

At the time that the swarm sallies forth, and that the bees are all in motion, the drones keep their place, and do not go out with the swarm, or if any attend the colony, they are only a few in number. But from the end of July to the middle of August, these drones are attacked by the common bees,

———*agmine facto*

Ignavum fucos pecus à præsepibus arcent.

Several bees fall upon a single drone and seize him by his wings and body; and although the drones resist as much as they are able, they are obliged to withdraw and leave the hive; they disappear so intirely, that we never could discover what was become of them.

When this sort of combat happens, you see all these animals in great commotion, as well without as within the hive. All the drones are so generally expelled, that of several hundreds, which we have often remarked in one hive, by the end of October

we have not found a single drone in several hives that we have examined.

Their origin is the same as that of the bees, they are the offspring of the queen, and produced in the same circumstances, with this difference alone, that the drones are lodged in separate combs made expressly for them.

We have said that in an hive there are combs whose cells are a third or an half bigger and longer than the common cells. The pregnant bee chuses these great cells for depositing there, with all the same state and attendance, as we have mentioned with regard to the common bees, those eggs from which the drones are to be hatched, and which cannot be distinguished by the sight from the common eggs; but it is probable that the mother hath some exquisite *feeling* to distinguish them, since she allots them abodes, proportioned to the size they are to have when they are inclosed in the cell in their utmost perfection. The drones undergo the same metamorphoses as we have remarked of the bees; they are as many days before they fall forth from their cells, and become *recluses* after the eighth day from their birth.

Lastly, They are nursed with the same care and tenderness as the common bees: But it is surprising that this attention and love which the bees have for those *little ones*, should be turned, as I may say, into so great an

hatred at the end of summer. This hatred is so universal, that it spares not even the young drones, who are yet only worms or nymphs included in the cells; for we have often remarked that at the time that a part of the bees have been expelling the great drones from the hive, there were other bees employed in unstopping the cells where the *imperfect* drones were inclosed, in dragging them out of the cell, in killing them, and throwing their bodies out of the hive; where we have sometimes seen two or three hundred killed of different ages.

Memoirs 1712, pag. 299.

Mr. Maraldi, by a nice examination of the internal structure of the drones, which differs in some respects from that of the bees, discovered some resemblances to the male organs of generation; and from thence conjectures that they are the males of the bee-insect; yet he could never discover them in the act of copulation. The ancients were inclined to think that there was no conjunction of sexes amongst bees.*

Illum adeo placuisse apibus mirabere morem,

Quod nec concubitu indulgent, nec corpora segnes

G 4

In

* *Whereas the common bees are of neither sex.*

In venerem solvunt, aut foetus nixibus edunt:

Verum ipsæ è foliis natos et suavibus herbis Ore legunt : ipsæ regem parvosque Quirites Sufficiunt, aulæsq; et cerea regna refingunt.

And Pliny observes,

Apium enim coïtus visus est nunquam. Plures existimavere oportere confici floribus compositis aptè atque utiliter *.

At last however, that sagacious enquirer Mr. de Reaumur, from whom nature can scarce conceal any thing, hath, in a great measure, removed the veil.

Some observations on bees by Mr. de Reaumur.

Mr. de Reaumur divided a great number of bees into *two hives*, of which the *one* had a queen, and the *other* had none. The queen did not at first appear to be much respected in her hive, but by degrees her subjects came to acknowledge her, to give her the royal retinue, to caress her, and to lick her with their proboscis, which is a pleasing and useful homage, as she is made more sleek by it, and her bronchiæ or stigmata are more opened for the air. Mr. de Reaumur judges with a great deal of probability, that the trouble and confusion, with which the separation of the

* Swammerdam had a notion that the female was fecundated without any venereal congress; that it was sufficient for her to be near the males; that a vivifying aura exhaling from the body of the male, and snuffed up by the female, might impregnate her eggs.

the two hives had been necessarily attended, was the cause of the little attention that had been paid to the queen at the first, when each thought of nothing but *himself*.

The two hives were very unequal in size and unequally peopled by bees. The *small* one had four or five times fewer than the other, but it was *there* the queen resided. In this hive they began industriously to work on combs, yet in a little time it was deserted by several bees who chose to go and settle elsewhere, perhaps on this very account because they had a queen; they were sensible that this queen would be too prolific for the hive, which could not contain a sufficient number of cells for all her young.

On the other hand, the bees of the larger hive came to settle in the small one; which was grown unpeopled although the *court* resided there, and they came thither in such crouds that not being able to enter *all* notwithstanding their utmost efforts, they heaped themselves on the outside in great clusters. Some subtil instinct had taught them that a queen was *there*, and some very urgent reason conducted them thither.

As this queen might be their mother, or at least their sister, one may attribute to them some particular affection for her, but Mr. de Reaumur hath been well assured that every queen is alike to them, and that they are attached, not to the *person*, but to the
dignity,

dignity, which is but too commonly true with regard to men.

The larger hive, in which there was no queen, had a fate far different from the smaller one. Scarce could the bees determine with themselves to go out and seek their sustenance in the fields, several of those that did go out disdained to return, no labour within no waxen edifices or stores, they pined away at last all died; and all this calamity for want of a queen, that is to say, if we may dare to surmise the thoughts of insects, for want of having hopes that a numerous issue would survive them. The displeasure of being deprived of posterity went so far as to make them neglect life, which may appear perhaps heroical. It was evident that the *small hive* displeased very much, on account of its smallness, the bees that were lodged in it yet *there* they built their combs and formed their works; for *there* a queen resided:

— *Rege incolumi mens omnibus una est;
Ille operum custos.*

When an hive full of bees, amongst which there was a queen, was soaked * in water. Mr. de Reaumur observed, that as soon as the queen was the least revived, all the others who

* Note, The motions of bees being too lively and quick, to make the necessary observations; Mr. de Reaumur contrived a method, by half drowning them in a tub of water, to perceive distinctly the minutest circumstances, as they recovered slowly to life and motion.

who were no more recovered than her, forgot themselves, and were employed on that important personage, contributing all in their power to restore her.

There is however some foundation to suspect *that heroic love* in bees, either for their queen or for their posterity, to be only at the bottom a self-interestedness. It is of great moment to them to be, even in their life-time, a mighty people; the cold weather would destroy them, if their great numbers did not sufficiently warm the hive, and their numbers depend on the fruitfulness of the mother.

They proportion their work to the profickness of the mother. Mr. de Reaumur observed an hive, where they were at work on a few combs, and with little vigour. From whence this inactivity and dispiritedness so uncommon amongst bees? He saw the reason of it when he got into his hands the mother of the hive, who was small and pitiful in comparison to other mothers. The rest judged of her as she deserved.

What is still more surprising is, that a cell, which was known by its size and shape to contain an egg from whence a mother naturally should be hatched, having been conveyed into another hive, which was designedly deprived of its mother, the bees of that hive immediately perceived that they had, at least, the hopes of a mother, and set themselves

selves to work ; but on the footing *alone* hopes, and did not apply themselves to it with all their natural vivacity till the mother was born, who indeed appeared very likely to answer their expectations.

As the queen hath her title only because she is the mother of all the people, the bees are attached to her only as she is a mother, and not as she is a queen. They pay her a kind of filial duty, that is proportionate to her fertility and usefulness ; if another queen comes into the same hive, they will respect her as much as the former, and easily suffer the royalty to be divided ; they receive with pleasure the assurance of a more numerous posterity.

One would think that where multiplication is so honourable, the mysteries of love should not be very secret ; yet how diligent soever the naturalists have been in peeping behind the curtain, they have never discovered the consummation of nuptial rites : Even Mr. de Reaumur himself could see no more than to raise a jealousy, and to give strong suspicions. Having stood the fiery trial of so many prying eyes in every age, the bees had gained the character of an inviolable chastity ; but Mr. de Reaumur hath, with *learned barbarity*, intirely blasted their reputation ; he makes the queen no better than a Messalina*, or, to compare her to one of

* Vid Juvenal. Satyr. vi. ver. 128.

her own dignity, another Cleopatra. He
at a drone and a female bee in private to-
gether, the drone appeared to be very cold
and indifferent, and, contrary to what one
would expect, it was the female that made
all the advances, a thousand tender ca-
resses—*The rest I beg leave to give in the lan-
guage of Ovid,*

*Pugnacemque tenet, luctantiaq; oscula carpit,
Subjētatq; manus, invitaq; pēctora tangit;
Et nunc hac juveni, nunc circumfunditur
illac.*

*Deniq; nitentem contra, elabique volentem
Implicat, ut serpens, quam regia sustinet
ales.*

The experiment was repeated and varied
several times, but always the like coldness in
the males, and the same ardour in the fe-
males. The adventure hath often a tragical
end with respect to the males, they die, and
one cannot assign any reason for it unless it
be for shame.

How Mr. de Reaumur escaped having his
eyes pulled out by the Fair in France I know
not, but he saw and hath blabbed secrets of the
queen in French, that I would not for the
world translate into English; that philosophers
however may not be angry with me for omitting
a natural curiosity, I shall give the substance of
what

what Mr. de Reaumur hath said in one line
Horace,

*Clunibus aut agitavit equum lasciva sup-
num.*

The fertility of the mother bees, even such as are the most teeming, is suspended during winter, and moreover an hive always loses a great number of its inhabitants, either by cold or by hunger. At the return of the spring the mother resumes her employment of laying eggs, by virtue of an impression that she must have received six months before. The eggs of the bees therefore are not, as in the greater animals, destined by nature to be emitted *all* at a certain determinate and nearly equal time after their fecundation. They acquire the *maturity* that is necessary to them in very unequal times; probably they cease to acquire *any* during the whole winter.

By Mr. de Reaumur's calculation, a queen may lay two hundred eggs a day in the most favourable seasons, which may include more than an hundred days in the year. It is not surprising therefore that an hive should no longer be capable of containing both its ancient inhabitants and the new-born progeny; from thence great numbers are obliged to go forth and establish themselves elsewhere: This migration is called a *swarm* and the hive is said to have *bred*. When a swarm

swarm, that seeks its fortune, goes to seize on a place already occupied by other bees, on a hive well peopled; it often finds a very vigorous resistance, and the great battles that are fought either by bees or men arise in general from such occasions.

A swarm doth not go out unless it hath a queen at its head,

— *ducunt examina reges;*

This is so true, that Mr. de Reaumur having been surprised to find no swarm to issue from an hive so well peopled, that it must have become too thronged an habitation, and suspecting that the want of a queen was the cause of it, found in reality by soaking the whole hive in water, that a new colony could not have a commander to lead them, or more properly a mother to assure them of that posterity which they are so passionately fond of.

When you see the works carried on faintly in an hive, it is a mark that there will soon be a swarm. It seems as if the resolution had been taken in a general council of the nation, the day fixed, and that till then it was sufficient to supply the actual wants of life.

Four or five days after a female is born, she is in a condition to be a queen and to command a swarm. As all her dignity consists in her fruitfulness, it is highly probable that

that she quits her *minority* as soon almost as she quits her nest, and makes herself capable of wearing the crown by the aid of the drones ; for it doth not appear that any drones go forth with the swarms, at least they do not with all swarms.

It may happen that a swarm might be composed only of an intirely new generation, but that is not the constant rule ; old and young bees go forth together in order to found a colony : You may distinguish their age, almost with certainty, by their colour alone ; the old are more reddish. The queens themselves are in this respect like the others. Most commonly a swarm is conducted by a young queen. But it is possible that it may be led forth by two, or even by three. What will happen in that case ? The reader perhaps would not have divined it, but it will cost their life to the supernumerary queens ; there will remain no more than *one*. Is it the common working bees that have destroyed the supernumerary sovereigns ? Or hath there happened a combat between the rival queens ? It is most probable that the first is the cause. The bees are desirous of posterity, it is their only wish, but then they would have it proportionate to their provision for them. A greater issue would occasion fatigue to themselves, and perhaps unhappiness to their offspring. They chuse rather to prevent these inconveniences by
rid-

ridding themselves of some queens, whom they consider as made only for the common good, and that ought to be sacrificed to it when occasion requires.

If this great reason did not demand that sacrifice, several queens would be suffered in the same state; which sometimes happens, and even foreign queens, to whom they had not been accustomed.

The first fifteen days of the new establishment of a swarm in an hive are employed in the most active labour, there is sometimes as much work dispatched in that little time, as in all the rest of the season that is proper for working.

There may go forth three swarms from a single hive in one season; and there have been swarms that consisted of forty thousand bees.

An hive that is tended with the usual care must perish in the course of a few years by divers accidents happening to the bees. Mr. de Reaumur mentions an hive that was preserved for thirty years; the case must be rare; but nine or ten years might, with care, be very possible.

History 1740, pag. 22.

Virgil indeed says,

Ergo ipſas quamvis anguſti terminus ævi
Excipiat, (neque enim plus ſeptima duci-
tur æſtas)

VOL. II.

H

Yet

Yet Aristotle allows they may live to nine or ten years, and Columella expressly says,

Durantque, si diligenter excultæ sint, in annos decem.

And Pliny likewise observes,

*Alveos nunquam
Ultra decem annos durasse proditur.*

Observa-
tions on
spiders, by
Mr. Hom-
berg.

The colour and extraordinary shape of a certain species of spiders that I have met with in a garden at Toulon, amongst the flowers of tuberoses, which were there in great abundance, have given me the curiosity to examine with care their external *figure*, and likewise *that* of all the other species of spiders that I could find. I have made use of a microscope to discover certain parts, which the eyes *alone* are not capable of discerning; and I have got them *drawn* much larger than the life, in order to represent them to the reader as they have appeared to me in looking at them through the microscope.

I shall give only the description of six principal species of these insects that I have seen, and to which all the others that I know of may be referred.

The six different species are, first the house-spider, that is to say, the spider that makes her web on the walls, and in the corners of apartments; secondly, the garden-spider, or
I she

the that makes a web in the open air, of a roundish figure and of a loose texture, and that nestles in the day-time in the center of her web; thirdly, the black cellar-spider, that dwells in holes of old walls; fourthly, the wandering-spider, which doth not keep quietly in a nest like the other spiders; fifthly the field-spider, that hath very long legs; and sixthly, the enraged spider, or the famous tarantula.

I thought it proper to give at first a description that would suit, in general, all the species of spiders, and to point out afterwards the particular characters of each of these species. I do not design to give an exact description of all the external parts of this insect; I shall only relate what cannot be well discerned by inspection alone, without the aid of a microscope.

The whole body of the spider may be divided into a fore-part, an hinder-part, and feet. The fore-part contains the breast and head; the hinder-part composes its belly. These two parts are connected together by a stricture, or by a very small ring. The greatest part of spiders have their fore-part, or the head and breast covered with an hard or scaly crust; and the belly, or hinder-part, always covered with a supple skin. The feet are fastened to the breast, and are hard like the fore-part of the body. This *structure* is different from *that* of several other
H 2 creep-

creeping and flying insects; which have the belly and breast united together, without any stricture, notwithstanding that the breast is covered with an hard crust, and the belly with a soft skin.

All spiders are covered with hairs, as well their hard parts as the supple.

They have on different parts of the head several eyes, which are very distinct, of different sizes, various in number, and variously placed. These eyes are all destitute of eyelids, and are covered with an hard, smooth and transparent crust.

They have in the fore-part of the head a kind of claw*, resembling in some measure the claws of crawfish, which makes, together with the front of this animal, the whole fore-
 Plate 3d, part of its head (*vid. fig. 1, 2, and 3*). This forceps consists of two branches a little flat, and covered with an hard crust; they are fastened perpendicularly to the inferior part of the forehead by a supple skin, which serves them for an articulation or hinge to open and shut the forceps. These branches are furnished with very hard *points* at the two ends; they serve to catch their prey and to bring it to their mouth, which is situated behind the forceps.

The inferior extremities of the forceps have each an hooked nail, resembling in some measure the nails of a cat. These
 nails

* Or forceps-

nails are large, very hard, and articulated; insomuch that the animal can move them upwards and downwards, without having need to move the branches of the forceps.

All spiders have eight legs articulated in the same manner as the legs of crawfish; they have at the extremity of each leg two great nails hooked, and articulated.

There is at the extremity of each leg, between the two nails, a bundle resembling a sponge a little wetted, like to what is observed at the extremities of the feet of flies. This *spungy bundle* serves probably for the same purposes as *that* of flies, *namely*, for walking perpendicularly up polished bodies, such as mirror-glass, where the hooks at the extremities of their feet must be useless: But these sponges furnishing a liquor a little gluish serve to fasten them. As the animal grows older this gluish liquor dries up both in spiders and in flies, insomuch that they are not able to walk a long while perpendicularly on mirror-glass; and an old spider or an old fly having fallen by chance into a china jar, which is pretty deep, will not be able to get out, but must die *there* by hunger.

The same thing almost happens to spiders with regard to the matter that furnishes their web. An old spider hath no more of this matter in its body, and cannot repair its web that hath been broken or carried

away ; it is obliged to chase a spider of the same kind, in order to get a nest to inhabit, as I have often observed. Perhaps the liquor of the extremities of the feet is the same as that of which the web is formed, or at least analogous to it, since by age they dry up pretty nearly alike.

Spiders have besides the eight legs we have been mentioning, and which serve them for walking, two other legs still nearer the head, with which they do not walk, but which serve them for arms and hands, to place and turn their prey that is held within their claws, for presenting it in all manner of ways, and in different directions to their mouth, which is placed immediately behind their forceps. This fifth pair of legs, or these arms, are not made in the same manner in all the species of spiders ; in some they resemble perfectly the other legs, and in others they are intirely different from them.

There are round the anus of all spiders four *papillæ*, or little muscular nipples, large towards their bases, and pointed at their extremities (*vid. fig. 7.*) These nipples have a very free motion in all directions. From the middle part between these nipples there issues, as from a wire-drawer, the gluish liquor that produces the thread, with which they make their webs and nests. This wire-drawer hath a sphincter, for opening and for contracting itself ; by which means they can spin

spin groffer and finer threads ; and the spider, being suspended in the air by this thread, keeps hanging whilst its wire-drawer is contracted ; but continues to descend by its own weight when the wire-drawer opens.

We shall now describe the manner in which spiders fabricate their webs. When a spider performs this work in some corner of a chamber, and she can easily go to all the places where she would fasten her threads, she spreads open the four nipples of which we have been speaking, and at the same time there appears, at the opening of the wire-drawer, a very little drop of that gluish liquor which is the substance of these threads : She presses with force this little drop against the wall, the drop fastens to the wall by its own natural glue, and the spider in retiring from that place lets run from the hole of her wire-drawer the first thread of the web she would form. As soon as she is arrived at that part of the wall where she would terminate her web, she impresses with her anus the other extremity of the thread, which fastens there in the same manner as she had fastened the former end ; then she removes about the space of half a line from the first thread she hath drawn, and fastens a second thread, which she draws *parallel* to the first.

*Who made the spider parallels design,
Sure as De-moivre, without rule or line?*

Having continued this second thread to the place where the first begun, she fastens it to the wall; and thus continues to draw the parallel threads from end to end till she hath given her web the breadth that she designs. All these parallel threads may be called the *warp* of the web. After this she goes across these ranges of parallel threads, fastening in like manner the one of the two ends against the wall, and the other end perpendicularly upon the first thread that she had drawn, leaving by this means, intirely open, *one* of the sides of her web, like a net, in order to give a free admittance to the flies that she would intrap. One may give the term of *woof* to those threads that go across the first parallel threads, which we have called the warp. As these threads freshly spun glue themselves to every thing they touch, they easily fasten crosswise one upon another, which causes the firmness of the web; whereas the firmness of cloths that we make for our uses consists in the texture or interweaving of the threads of the woof with those of the warp; which is a manner of working that is the result of reason.

In order that the threads which run crosswise might be glued together with greater firmness, the spider pats them with the four nipples of her anus, and compresses in different

ent directions all the *places* where the threads cross each other, in proportion as she lays them one upon another: She *triples* or even *quadruples* the threads that border her web, in order to strengthen them and hinder their being easily torn.

A spider can supply two or three *times* materials for making a new *web*, provided that she hath not made *one* too large at the first, which might have exhausted the matter of these threads; after *that*, if she wants a web, she must either occupy by force the web of another spider,

—*et vivere raptō,*

or she may find some deserted web; for the young spiders quit their first webs and make themselves new abodes; but if the old spiders, I mean the *house*-spiders, neither *seize* nor *find*, they must necessarily perish, for *they* cannot live without a web. There are some other species of spiders that have not so much occasion for a web.

With regard to the garden-spider, that strings her web in the air, her manner of working is *thus*; she places herself in calm weather at the end of some branch of a tree, or on some other body that juts out; she fastens herself *there* by her six feet, and with the two hinder feet she draws from her anus, by little and little, a thread of the length of two or three ells or more, which
she

she lets float in the air till the wind has driven it against some solid matter, where the thread fastens itself quickly by its natural glue: The spider draws from time to time the thread *back* to herself, in order to know if the end that floats in the air hath fastened any where, which she perceives by the resistance that she feels when she draws the thread,

*The spider's touch, how exquisitely fine !
Feels at each thread, and lives along the
line :*

The Pendets (who are Indian gentiles) make the spider a kind of symbol of the Deity; they " feign that a certain immense spider " was the first cause of all things ; which, " drawing the matter from its own bowels, " wove the web of this universe, and disposed it with wonderful art ; she in the " mean time sitting in the center of her " work, feels and directs the motion of every part : till at length, when she has " pleased herself sufficiently in ordering and " contemplating this web, she draws all the " threads she had spun out, again into herself ; and, having absorbed them, the universal nature of all creatures vanishes into nothing."

Vid. Universal History, octavo, vol. I. p. 48.

The spider then draws the thread a little tight, and fastens it by the nipples of her anus to the place where she is. This thread serves her as a bridge or a *slack rope* for going to the place where chance had fixed it; by this means she doubles the first thread, even triples and quadruples it according to her instinct, or rather according to the length of the thread which requires to be more or less strengthened; next she places herself pretty near the middle of this thread, and draws from her anus with her two hinder feet a new thread, which she lets float in the air, and when she perceives that this new floating thread is fastened somewhere, she draws it a little tighter; then squatting her anus, she fastens with her nipples the end that she *holds*, as perpendicularly as she can, upon the middle of the first thread, and strengthens it by doubling or tripling it, as she did before. She doth *this* so often, that the middle of the first thread becomes a center, from whence issue several *radii*; which she continues till she can walk upon threads from the extremity of one of the *radii* to the extremities of the others: Each *radius* likewise she strengthens by additional threads, which she places along *them*, and makes as *many* as she finds convenient. All the *radii* being finished, she nestles herself in the center of her web, with her head always downwards, perhaps in order to avoid the sun-shine, having

ing no eyelids to modify it; or rather to rest her big belly upon her broad *breast*, where her legs are *inserted*, which expanding themselves support the whole animal; whereas if the head was upward, the belly, which is extremely big, could not be so conveniently sustained.

The spider keeps in the center of her web only in the day-time: She retires at night, or when it rains, or when there is an high wind, into a little lodge that she hath made herself at the extremity of her web, under the leaf of a tree or a plant, or in some other place more solid than her web, and which may shelter her from rain. She commonly chuses this place towards the most elevated part of her web, probably that she may have refuge to it speedily in case of necessity; for the greatest part of spiders ascend very easily and much quicker than they descend.

The spider sits watching for flies or some other insects that come and intangle themselves in these toils, and which serve her for food. When the fly is small, the spider takes it in her forceps, and carries it to her nest; but when the fly is somewhat big, in comparison of the spider, and with its wings and feet might incommode her, she *then* surrounds and envelopes it with a great number of threads which she draws from her anus, in order to bind and tie, as it were with
8 cords,

cords, the fly, till it can no longer stir either wings or feet, and the spider carries it peaceably to her nest and feeds upon it. Sometimes the fly is so big and strong, that the spider cannot get the better of it; in that case, far from embarrassing the fly any longer, she lets it go, tears the place of her web where the fly is suspended, throws it out, and immediately *after* repairs the web she had torn, or makes an intire new one.

All the *male* spiders are much *smaller* than the *females* of their kind. The difference extends so widely, that I have weighed five or six *male* garden-spiders against one *female* of the same kind, to find its equal weight; which is a very common thing in the greatest part of insects, quite contrary to quadrupeds, whose males are much larger and stronger than the females.

Spiders of all kinds are oviparous, with this difference, that some lay a great quantity of eggs, such for instance are the garden-spiders, and those that are commonly called spinners; others lay very few, as the house-spiders, &c. They lay their eggs on a portion of their web which they wind into a bottom, and *fit on* them in their nests. When one drives them from their nests at the time they are *sitting*, they take this clue of eggs in their forceps, and carry it away with them. As soon as the young are hatched, they begin to spin; and one may almost *see* them

them grow, yet I could never perceive them to take any nourishment. If by chance there comes to them a very little fly, they throw themselves upon it, and act as if they fed on it: But if there doth not come any to them for a day or two or more, they do not cease to grow in the same manner as if they had taken nourishment.

The particular characters of each sort of spiders consist in the different position of their eyes; yet we shall remark several other considerable differences, but which are not so general.

The house-spider, which makes the first sort, hath eight eyes placed on its front in an oval. These eyes are small, and are all pretty nearly of the same size (*see fig. 1st*). This spider, as we have already observed, makes a wide-spreading web in the corners and against the walls of chambers: Her arms resemble perfectly her legs, except that they are a little shorter, and that she never puts them to the ground. This species of spiders *cast* their skin every year, or change both skin and feet, like the crawfish; which I have never observed but in this single species of spiders. She lives a long time; I have seen the same spider for four years: She did not grow much bigger by age in her body, but grew a great deal in her legs. There sometimes happens to this sort of spiders a disease that makes them appear horrible:
They

They become quite full of scales that do not lie flat one upon another, but the animal is bristled with them; and amongst these scales you find a great number of little insects resembling the lice of flies, but much smaller. When the diseased spider runs a little quickly, she shakes off a part of these scales together with the little insects. The disease is uncommon in our cold climates; I never observed it any where but in the kingdom of Naples. The spider in this condition doth not continue long in the same place; and if confined she dies very soon.

The second species is the garden-spider, which makes a great round web in the air, of which she commonly occupies the center: She hath four great eyes placed in the middle of the forehead *squared*, and two smaller eyes on each side of the head (*see fig. 2.*) The females of this *sort* have the biggest bellies, that I have seen in spiders, the males are very lank: They are of different colours, but commonly a *fillemot*, spotted with white and grey; sometimes they are all white, such were those that I found at Toulon amongst the flowers of tuberoses. I have found some likewise of different green colours; they are not all of the same bigness: The green are the smallest, the white are bigger, and the grey are the biggest of all. I have poured spirit of wine upon this species of spiders, they did not appear to be
the

the least incommoded by it, no more than by *aqua fortis* or oil of vitriol; but the oil of turpentine killed them in an instant: Which is a method I have often practised for destroying the nests of young spiders of this *sort*, in which one finds sometimes an hundred at once, that in a few days get possession of a whole garden, and spoil abundance of plants.

The third species is the *cellar-spider*, and *such* as make their nests in old walls: They appeared to me to have no more than six eyes, all the other species having eight. These eyes are placed *two* in the middle of the forehead, and *two* on each side of the head, and are all six pretty nearly of the same size (*see fig. 3d.*) The spiders of this *sort* are all of a black colour, and very hairy: They have short legs, are stronger, more mischievous, and live longer than the greatest part of the other spiders. When you have taken hold of *one*, she defends herself, and bites the instrument that holds her; although pierced through the belly, she lives sometimes above eight and forty hours; whereas all the other spiders die soon when pierced through the body, and do not defend themselves or ever bite when taken. Instead of a web to catch flies, *these* do nothing but *draw out* threads seven or eight inches long, that *extend* from their nests like so many radii, and *fasten* to the wall which is round

round the hole of their habitation : The insect that walks on this wall and strikes against some one of these threads, by shaking it a little gives notice to the spider that is in her hole ; who sallies out in an instant with extraordinary swiftness, and carries away the insect. I have seen a very lively wasp seized by one of these spiders, which the other sorts of spiders will not attack, as well through fear of the sting as on account of the hard scales with which the whole body of the wasp is covered : But the anterior part and the legs of this sort of spider being covered *themselves* with an extremely hard scale, and the posterior or belly being defended by a thick and very compact skin, they do not fear probably the sting of the wasp ; and the forceps of this spider being very strong and hard, they are capable of breaking the scales of the wasp.

The fourth species of spiders consists of those which we have called *wanderers*, because they are not sedentary in their nests as all the other spiders are, who quietly *watch* for their prey ; but these go out to seek their prey and chase it with a good deal of cunning and artifice. They have two great eyes in the middle of the forehead, two smaller at the extremities of the forehead, two of the same size on the back-part of the head, and two very small between the forehead and the back of the head (*see fig. 4th*).

The spiders of this species are of different sizes and of different colours; I have seen white, black, red, grey, and speckled. They have a part of their body different from all the other *sorts*, which is that the extremity of the fifth pair of legs which we have called their arms, is terminated in a plume of feathers, whereas in all the other spiders it is terminated in two hooks like the rest of the legs. This plume is commonly of the same colour as the rest of the body of the animal, and equals sometimes in size the whole head. The use of it is to *throw* upon the wings of the fly, which the spider hath caught, in order to prevent its *rising*; for this sort of spider hath not the same means as the others of embarrassing the fly, and intangling it with nets of her own making.

The fifth species is the field-spider, which is commonly called the spinner. This sort hath the fore-part, *namely*, head and breast, flat *horizontally*, and almost transparent, being covered with a very fine, sleek, and whitish scale. There is a great black spot on its head, which I take to be the brain, that appears through the transparent scale, which covers it. This spider hath eight eyes *placed* in an extraordinary manner; there are two in the middle of the forehead, very small and very near each other, insomuch that you might take *both* for *one* little oval body. At the extremities of the
fore-

forehead, to the *right* and *left* there are two little bumps, and on the summit of each of these bumps there are three eyes placed very near each other (*see fig. 5th.*) These eyes are bigger than those of the middle; they have a very protuberant cornea that is white and transparent, though the *fundus* is black, whereas the two eyes in the middle are throughout black. You see very plainly a duct run from each of these bumps, and likewise from the two eyes in the middle. These three ducts *terminate* in the black spot which I take to be the brain. In proportion as these ducts remove *further* from the eyes, they draw *closer* to each other so as to enter nearly at the same place of the brain. These ducts contain probably the optic nerves. The legs of this sort of spider are very slender, and much longer in proportion than the legs of the other spiders; but their arms are extremely short and fleshy, by no means *resembling* the legs, as they *do* in the greatest part of the others. Their legs are so full of hairs, that they appear through the microscope like quills.

The sixth species of spiders is the famous *tarantula* (which derives its name from *Tarentum*, but it is found in other parts of Italy, and particularly in Apulia.) It hath the gate and figure pretty nearly of our house-spider, but is in all its parts much more strong and robust: It hath the legs and under-part of the
 I 2 belly

belly spotted white and black : But the upper-part of its belly, as well as all its anterior part are black : Its head and breast are covered with a single black scale, which resembles perfectly a small tortoise. The spiders of this *sort* have eight eyes, which are intirely different from those of the other species of spiders, as well in colour as in consistence. All the eyes of the other species of spiders are *black* or of a red inclining to a black, and are covered with an hard and transparent scale that continues such even after their death : But these are covered with a *cornea* that is moist and tender, *which* withers and sinks *in* after their death : The colour of *it* is a white inclining a little to a gilded yellow, brilliant and sparkling like the eyes of dogs and cats when seen in the dark. These eyes are situated four in a *square* in the middle of the forehead, and four in an *horizontal line* : Underneath the four *first* the four *last* border the bottom of the forehead, and are placed immediately above the root of the forceps. These eyes are different in size ; the four first are pretty nearly of the same bigness, they have about a line diameter, and are very visible without a microscope ; but the last have not more than half of the diameter of the first. The tarantulas are very mischievous, and bite eagerly when provoked. I have seen them at *Rome*, but *there* they are not feared, because

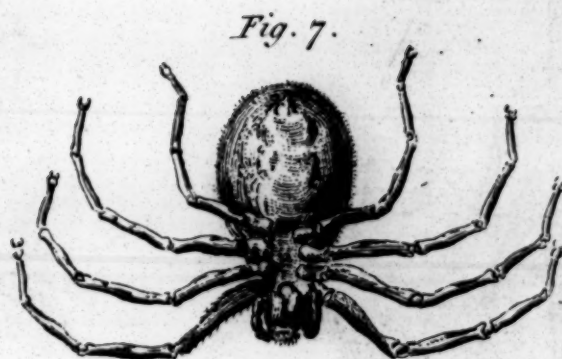
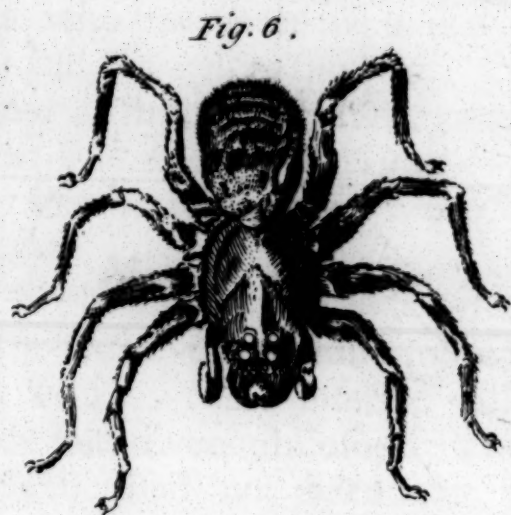


Fig. 1. Represents the Eyes and Talons of the House Spider.

2. The Garden Spider, that keeps in the Air in the midst of her Web.

3. The black Spider, that dwells in the holes of old Walls.

4. The Wandring Spider, that doth not keep in a Nest like the other Spiders, but goes in pursuit of flies and other insects.

5. The Head and Eyes of the Field Spider, commonly call'd the Spinner.

6.6. The Tarantula.

7. A Spider inverted, to shew the Papillæ or Nipples of her Anus, which she makes use of for Spinning.



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cause there is no instance of their having in-
commoded any one in that place: Whereas
in the kingdom of Naples they do a great
deal of mischief, perhaps because it is hotter
there than at *Rome*. Mr. Geoffroy observes,
in general, that they are innoxious on *hills*,
but venomous in *plains*, on account of the
greater heat. The symptoms that happen to
those who have been wounded by them are
very odd, as well as the cure; they have been
treated of by several Italian and French au-
thors: *But by none so well as by the illustrious*
Dr. Mead in his excellent treatise on poisons.

Memoirs 1707, pag. 339.

The hatred of mankind to spiders had long
been fatal to these insects: All the curious
things that divers philosophers had published
of them, had not been able to reconcile
them to the mind; they passed continually
for a dangerous insect, or at least useless,
when Mr. Bon, first president of the Chamber
of Accounts at Montpellier and honorary
member of the Royal Society in that city,
drew a very general attention on an animal
so universally hated: Indeed there was rea-
son to hope, from the singular things he dis-
covered, that one might derive some ad-
vantage from them, since spiders spun, like
worms, a filk, of which it was plain there
might be made very beautiful manufactures.
The stockings and the mittens that he pre-
sented

Observa-
tions on
the inge-
nious dis-
covery of
Mr. Bon
with re-
gard to the
filk of spi-
ders, by
Mr. de
Reaumur.

sent to the assembly were an incontestable proof of it; they had been made, under his care and direction, of this new-invented silk. This Academy, to whom he sent some time *after* the mittens, saw them with that pleasure which she always receives from things *curious*; but the particular attention of this company to whatever concerns the public good, did not permit her to stop *there*. She thought it her duty to examine more strictly a discovery that had some appearance of utility, in order to draw from it all the advantage which could be derived, or at least to let the world see that she had listened to the courting flattery of advantages. She knew too well the fate of the silk from silk-worms, which though known continued almost useless for several ages, not to fear lest the silk of spiders should suffer a like fortune.

Pliny in speaking of the silk-worms observes,

Telas araneorum modo texunt ad vestem luxumque sceminarum, quæ bombycina appellatur. Prima eas redordiri, rursusque texere invenit in Ceo mulier Pamphila, Latoï filia, non fraudanda gloriâ excogitatae rationis, ut *denudet* sceminas vestis.

Nec puduit has vestes usurpare etiam viros, levitatem propter æstivam. In tantum à loricâ gerendâ decessere mores, ut oneri sit etiam vestis.

In the time of Aurelian, silk was fallen into great disuse, (as appears from the excessive price it bore, being sold for its weight in gold) perhaps on account of the effeminacy of the dress. If this had been the motive of that truly great and honest minister the duke of Sully, the vehement opposition he made to the introducing silk-worms into France would deserve applause; but his view seems to have been chiefly political, thinking it would not turn out to the advantage of his country. In this political view the master shewed a much greater penetration than his minister.

The Academy thought proper to appoint two of her members to *trace* the ingenious discovery of Mr. Bon. I was one of those that she honoured with this commission; and in order to examine the silk of spiders in a regular manner, I thought it incumbent on me to consider *this* silk relatively to *that* of the silk worms, to endeavour to discover by this comparison if one might derive from the *new* silk any advantage equal to what we receive from the *old*: For the inquiry was not whether spiders spun at certain seasons a silk proper for manufactures; Mr. Bon had demonstrated that, in a manner as curious as it was certain; but whether they spun a silk by which the public might be profited? In order to determine this question, the whole seemed to me to be reduced not only

to find the secret of nourishing and breeding spiders; but to know besides whether the secret of nourishing spiders *being* found, this silk could be as cheap as the other; or in case it was dearer, whether this inconvenience would be compensated by some other advantage.

The artifice which spiders make use of for catching flies, proves that they feed on those insects; but the least reflection will serve to shew that it is not possible to nourish, with flies, so many spiders as would be necessary to furnish the silk manufactories. What an expence of labour to catch such a multitude of flies, even supposing them sufficiently numerous?

It was necessary therefore to have recourse to some other nourishment, of which one might conveniently have a sufficient quantity. The voracious disposition of spiders is an evidence that they would not be content with flowers, leaves, or fruits of trees: I did not forbear however to try this sort of aliment; that I might not reproach myself with having neglected any thing, and because I knew that in matters of experience, *that* often happens *which* one least expected to happen; but all that I gave them of this kind, was not a suitable food to them.

I was easily satisfied that *flies* were not the only food that might be given them; for although those that make their webs in the
corners

corners of walls and in gardens live on *them*, yet I had observed more than once that they equally eat other insects, which happen to be caught in their webs; those spiders that inhabit holes of old walls, had shewn me still further, that all insects were acceptable to them: For having often visited such holes, I had commonly found there the carcases of divers sorts of insects, such as wood-lice, caterpillars, butterflies, &c.

The only business remaining was to find a sort of insect, of which one might conveniently have *as many* as one would; and earth-worms alone seemed to me to offer this advantage. There are prodigious quantities of them, the gardens and fields are full of them; every one must have observed that after rainy nights the walks of gardens are covered with little round heaps of earth, turned in a spiral form, which conceal a like number of holes, out of which the earth-worms have issued. There is nothing, likewise more easy than to get these insects, provided you go and seek for them in the night-time with a candle, observing only to go thither at such times as have not been preceded by a long drought.

It is true that I had never found *earth-worms* in the webs or in the holes of spiders; but those insects creeping on the ground and having a good deal of strength and weight, it was equally impossible that they should

should be taken in these nets and holes, or that the spiders should have conveyed them thither. I thought there was no food of which I could promise myself a greater plenty. The success did not disappoint my expectation: Having shut up in boxes several large spiders of divers *sorts*, that had survived the winter, (for there are some that live several years) I gave them pieces of worms and preserved them alive by this means.

It would not have been sufficient to convince me that this nourishment was suitable to spiders, *to have seen them live* for several months after I had given it them: An experiment that I had made formerly would have left a well-grounded doubt. I had kept an house-spider alive for more than three months without giving it any food; and it is well known, that the young spiders which are hatched in the month of September, live about eight or nine months without eating any thing.

But as I had included these spiders in boxes covered with glass, I easily observed whether they attached themselves to the food that I had given them; and I saw them attack the pieces of worms, which, it is well known, move themselves notwithstanding the separation, in the same manner as one sees them attack insects, in whom there remains some strength after having been
2 caught

caught in their nets. The divers motions of these pieces of worms excited these insects of prey; and moreover they preserved their bigness and vivacity, which did not happen to those that I left without food. Lastly, which is more decisive, several of them spun cods in which their eggs were inclosed.

I tried afterwards other sorts of meat, which would have been still more convenient to give them than worms, such as raw flesh; but they did not seem to relish it, perhaps the reason is because the natural fierceness of spiders requires to be roused by *living* animals.

I contrived a sort of food that pleased them extremely, and particularly the young spiders. I plucked off feathers from birds, especially pigeons, and squeezing out the pith which was bloody, I offered it to them, and they sucked it with great greediness.

Hitherto all appears to go on extremely well. We have got very common kinds of food, which seemed to be the only difficulty; perhaps too we might find other foods as convenient, even amongst insects, whilst we are making use of the above-mentioned, which are not more difficult to find than the leaves of the mulberry-tree that are given to silk-worms, and which have this additional advantage, that they may be had without any trouble in all countries, and without fear of the severest winters. Poulterers

terers would furnish a prodigious quantity of young feathers, or you may pluck them from the backs of fowls and pigeons from time to time without doing any mischief, as I have experienced.

But we are now going to see that these promising hopes will be greatly abated by considering the number of spiders that it will be necessary to breed for supplying the silk manufactories.

As soon as the young spiders quit the silk that enveloped them, they appear in perfect harmony together, they work by concert on the same web; some extend new threads upon those that others had already furnished. But this union lasts not long; having put some hundreds together in a sufficiently large box, and supplied them with plenty of food, I soon found their ferocity to exceed the most savage quadrupeds.

—————*parcit*

*Cognatis maculis similis fera. Quando leoni
Fortior eripuit vitam leo? Quo nemore un-
quam*

*Expiravit aper majoris dentibus apri?
Indica tigris agit rabidâ cum tigride pacem.*

The biggest and stoutest spiders seemed to have a relish for eating the smallest and weakest, insomuch, that in a short time there did not remain above one in the box.

It is probable that this inclination they have for eating one another is, in part, the cause that there are so few spiders in proportion to what one would expect, for they lay a prodigious quantity of eggs: I have said *in part*, because I know there are divers sorts of insects that eat them; Pliny mentions some species of hornets and lizards that feed on them; and I myself have seen little brown wall-lizards catch them with a great deal of cunning.

There seems therefore no *other* way left, if one would breed spiders, *than* to lodge them *separately*. One might, for example, have boxes divided into several little compartments, which would form so many cells: And *this* I have attempted. But to give food to each of these spiders separately, would require expences greatly exceeding the profit expected from them. One might however contentedly undergo this labour, if we had not the silk of silk-worms in a manner infinitely more commodious.

The necessity there is of distributing the spiders into separate cells throws us again into a new difficulty, which doth not a little diminish the advantage that spiders have over silk-worms with regard to their fecundity. For, in order to *profit* from this advantage, it is necessary to keep a great number of eggs which have been fecundated by copulation, and for *that* you must necessarily put spiders

spiders *together*. I know very well that there is a season when there must be in these insects a *placid* fermentation, or as *Lucretius* expresses it in his address to *Venus*,

*Omnibus incutiens blandum per pectora amor-
rem*

*Efficis, ut cupide generatim sæcla propa-
gent;*

which soothes their natural ferocity, and one might *then* place them together without any risk; but how can one know precisely this season? It would be easy to find it if they all laid their eggs pretty nearly at the same time of the year, but there is a difference of several months between the times of their *laying*.

The fecundity of spiders is prodigious, as Mr. Bon hath justly observed; but then the *silk-worms* likewise are extremely teeming. Though we should suppose that *they* lay no more than about a hundred eggs, of which scarce forty produce worms that spin their cods, whereas spiders lay between six and seven hundred eggs; (yet I have observed in all the *silk-worms* which I have bred that they have laid between three and four hundred eggs) it is easy to see that one might multiply the number of *silk-worms* as much as one would, if their *increase* depended only on the quantity of their eggs: There needs no other proof of *this* than the quantity of
silk

filk which they furnish at present to Europe, where formerly there were no filk-worms; it would be easy in time to have a quantity of these worms surpassing as much what we have at present, as what we now have surpasses the little number that were originally brought from the east into Europe. But the reason that they are not more multiplied is, that it is necessary to lodge, to feed, to tend them, and by increasing the quantity of filk you would diminish the price of it, so that the care which is necessary to be taken for breeding filk-worms would no longer be sufficiently recompenced.

By what we have said hitherto it appears that the filk-worms have greatly the advantage over spiders, by the facility we have in breeding them, and consequently that one can hope but little profit from the new-invented filk.

I must observe that not *all* the species of spiders afford a filk that can be employed in manufactures, and that *those* who do furnish a properly conditioned filk, spin it only for forming the cods that envelop their eggs; for it is known that the nets which they spread for insects are commonly made of so fine a filk, that it would be impossible to make any use of it. I must add likewise, that the *filk* of spiders is not so strong as *that* of filk-worms; and although the diversity of spiders affords a variety of colours beyond
the

the filk-worms, yet the *lustre* of the spider's filk is far inferior to *that* of the filk-worm; which arises probably from the greater number of threads, of which it is composed.

I shall close this discourse by comparing the *quantity* of filk which each spider affords in a year, with *that* which is derived from the filk-worm. I have weighed with great care divers cods of filk-worms, and have found that the firmest, that is to say, *the product* of a whole year's work of each filk-worm, weighed four grains, and that the weakest weighed something more than three; so that taking sixteen ounces to the pound, there is need of at least 1920 filk-worms to get one pound of filk. When the *beau* flutters in his silks, he little thinks how many thousand worms have toiled their whole life to supply his luxury!

I have weighed with the same care a great number of the cods of spiders, and have always found that four cods of the biggest spiders were requisite to equal the weight of *one* from the filk-worm: So that there would be wanting four of the biggest spiders to give as much filk as one filk-worm, *upon supposition* that there was no more *waste* in the filk of the one than in that of the other, and that they all gave a genuine filk; but in fact the cods of spiders are subject to a great waste, from which the cods of filk-worms are exempted. What causes this waste

waste in the cods of spiders is, that they are weighed *full* of all the shells of eggs that inclosed the young spiders before they were hatched, and of divers ordures that happen to be mixed amongst the filk. If the waste therefore of these cods be subtracted, it will be necessary to *strike off* more than two thirds of their weight, since from thirteen ounces of spiders filk, with its filth, Mr. Bon got no more than four ounces of neat filk; whereas the cods of filk-worms have no *waste*, or it is so small that one might make a sufficient allowance for it by taking *that* of the spiders filk at two thirds only.

We have already seen that the weight of a spider's cod, before it is cleansed, is to the weight of a filk-worm's cod as *one* to *four*; it now appears that when it is cleansed, its *weight* will be to *that* of the filk-worm's as *one* to *twelve*. Therefore there must be *twelve* of the biggest spiders to yield as much filk as *one* filk-worm. Again, *every* filk-worm makes a cod, because it is necessary towards their metamorphosis; whereas spiders making their cods only in order to envelop their eggs, if we consider with all naturalists that have preceded Mr. Bon the spider species as consisting of male and female, I mean if they are not taken for hermaphrodites, they are only female spiders that make cods; from whence it *follows*, if

we suppose an equal number of males and females in our custody, which is most likely to happen, *that* 24 of the biggest spiders will give no more silk than a single silk-worm.

It would require therefore about 55296 *spiders*, and *those* of the biggest sort, to procure a pound of silk, all which spiders it would be necessary to keep and feed separately for several months.

The reader no doubt will regret that there remains so little hope of profiting from so ingenious a discovery. I must own however, that there is still a glimmering appearance of hope: Perhaps there may be found *spiders* that will give more silk than *those* which we see commonly in this kingdom. It is certain by the reports of all travellers, that *those* of America are much bigger than *ours*; from whence one would conclude that *they* make larger cods. The silk-worms, which although natives of far distant countries have *established* themselves so strong in Europe, would lead us to hope, that the spiders of America might live in this climate. At least it deserves our trial, which is the only means of discovering things curious and useful. For my part, I will neglect nothing that can contribute to the inquiry I have proposed, in which if ever any thing useful should be discovered, the first glory of it will be due to Mr. Bon.

In-

*Inventas aut qui vitam excoluere per artes,
 Quique sui memores alios fecere merendo,
 Omnibus his niveâ cinguntur tempora vittâ.*

Memoirs 1710, pag. 386.

Art as well as *nature* hath wonders that we often do not perceive, because they are continually before our eyes; content with satisfying our wants or luxury, we seldom think of *inquiring* to what ingenious contrivances we owe those things of which we make a common use. The *beau* bedaubed with gold, is seldom capable of knowing the real and truly valuable *riches* that adorn him; his conceptions and admiration scarce ever go beyond the tailor. Some philosophers indeed, have examined *them* with attention; they have drawn from thence excellent proofs of the prodigious divisibility of *matter*, to which they might have given still greater force, had they been more fully informed of all that the gold wire-drawer's art is able to perform: They have likewise sought to give the *reason* of the great ductility of metals, but *this* is one of the greatest secrets of nature. The cause of *ductility* is closely connected to the most obscure part of physics, I mean, to the cause of *hardness*, and hath besides *that* its particular difficulties. We own ourselves no more able than those *philosophers* to explain this property of bodies, but may,

Experiments and reflections on the prodigious ductility of divers substances, by Mr. de Reaumur.

without vanity, pretend to shew much further than *they* have done, how far art hath been able to take advantage of it: We have had opportunities of making divers experiments on this subject, we are going to relate them, and shall examine the effects of the ductility of very different bodies. The reader perhaps will not be displeased to find collected into one point of view whatever hath been discovered the most *singular* on a subject, in which art and nature seem to vie with each other in displaying wonders.

Ductile bodies *in general* are such as being *struck, pressed, or drawn out* extend themselves in one direction in the same proportion nearly as they diminish in another; such are metals which under the strokes of the hammer *acquire* in length and breadth what they *lose* in thickness, or which being drawn through a wire-drawing iron, become longer in proportion as they lessen in bigness. We have likewise another *sort* of bodies, which though they are not malleable like metals, may nevertheless be termed *ductile*; glues, gums, rosins, and all those bodies which having been softened by water, by fire, or by any other dissolvent, spin into threads, will furnish us with examples of this sort of ductility.

Ductile bodies may therefore be divided into two classes, of which the first comprehends those ductile bodies which we shall
name

name *hard*, and which are malleable; the second class is composed of ductile bodies that are *soft*, which one may extend by *drawing*, although they are not malleable.

The most common manner of *extending* the *hard* sort of ductile bodies is, by striking them with blows of the hammer; by such strokes well managed, the greatest part of workmen in gold, silver, copper, and in tin, give what figures they please to formless masses. Although these sorts of works deserve more attention than is commonly given them, yet *our* design is more extensive, and would consider ductile bodies with respect to the utmost extent that they are able to acquire.

They are gold-beaters principally who with the aid of the hammer alone make plates of metal extremely thin. They prepare for us those *leaves* that we employ in the greatest part of our gildings; it is well known that they deduce *them* from a large ingot, whose thickness they diminish to such a degree, that the leaves which are formed from it yield to the slightest puff of wind. *By this means gold like the Sibyl's verses, however weighty they were before, become as light as air;*

—*foliis tantum ne carmina manda,
Ne turbata volent rapidis ludibria ventis.*

In order to know with more certainty than can be obtained from the report of workmen,

in which Mr. Rohault reposed himself; to know I say, how far this art is able actually to extend gold, I took a certain quantity of the finest gold-leaf, such as is employed in gilding books; I with care measured the size, and weighed it in very nice scales; by this means I found that a grain of gold *beaten* (but what is a grain of gold!) had an extent of 36 inches square and an half, and 24 lines square; that is to say, that an ounce of gold which under the form of a cube would have no more than 5 lines and $\frac{1}{5}$ of a line at most, either in breadth, length, or depth, and would cover no more surface than about 27 lines square; that this ounce of gold, when it hath been extended by the gold-beaters, covers a surface of more than 146 feet square and an half; which is an extent near half as big again as what they were able to give to gold-leaf about 90 years ago.

It would take up too much time to explain *here* what hath the most contributed to bring this art to perfection; neither will we stop to unfold the artifice of *man*, which hath even ransacked the intestines of oxen for those leaves of a fine parchment, without which it would be impossible to reduce gold into such thin leaves.

I beg leave to observe that they are those parchment leaves taken from the intestines of oxen, which make the gold-beat skin that is applied to wounds.

But

But after all, how considerable soever the extent of surface in gold-leaf may be, it will appear to contain nothing marvellous, when compared with the extent that the same metal acquires from the gold wire-drawers. There are indeed gold beaten leaves which in some places have not $\frac{1}{30000}$ of a line in thickness, but $\frac{1}{30000}$ of a line is a very great thickness in comparison to the thickness of gold that covers the gilded silver in *brocaded* silks.

In order to shew how far gold is *then* extended, and how prodigious thin it is, it is necessary to have a general idea of the *process* of gold wire-drawers. The thread which we commonly name *gold-wire*, but is, as every one knows, only silver-wire gilt, is drawn from a large ingot of silver: They take this ingot at about 45 *marks* in weight (*that is 22 pounds and half of avoirdupois-weight*) by rounding it they form a cylinder or roller, which hath 15 lines in diameter, and somewhat less than 22 inches in length; they gild this ingot with gold-leaves prepared by the gold-beaters, thicker *leaves* are employed for this purpose than *those* that are designed for our common gildings; they often lay several of these leaves one upon another, yet after all, though the *stratum* of gold that covers this ingot is considerably thicker than *that* of our other gildings, it is still extremely thin: This will appear by

considering the quantity of gold that is made use of. In order to gild these 45 marks of silver they never employ more than 6 ounces of gold, this quantity suffices for the *super-gildings*; but they do not employ so much as two ounces, and oftentimes not much more than one ounce, when they want thread so slightly gilded as the common gold thread of Lions; that is to say, that the *stratum* of gold which envelops this ingot hath never more thickness than the 15th part of a line, that oftentimes it hath no more than the 30th or 45th part, and lastly that it hath sometimes only the 90th part.

Yet how much thinner still must this delicate *stratum* be reduced! how often, as I may say, must it be *subdivided*! they lengthen the ingot that is gilded till its *fineness* equals or surpasses *that* of an hair. They cause it to pass successively through holes much smaller one than another, or, which is the same thing, through wire-drawing irons. In proportion as it passes through an hole its diameter decreases, it gains in length what it loses in bigness, it increases consequently in surface, and the gold which covers this ingot of silver never ceases to gild it; how prodigiously soever you extend it, the gold continually pursues the silver,

—*expectas dum defluat amnis, at ille
Labitur, et labetur in omne volubilis ævum.*

Yet

Yet how many divisions must it have undergone, when the ingot reduced to a fine thread or wire hath a diameter about 9000 times smaller than what it had in the mass! but to give a more distinct idea of the prodigious ductility of gold, we will consider the *length* to which the ingot arrives when drawn to its utmost fineness.

I weighed out with great exactness an half dram of the finest thread or wire, and with the like care I measured the length of this half dram of thread; I found it to be 202 feet; by consequence an ounce of thread contains 3232 feet of length, and the mark or eight ounces contains 25856. Our ingot that weighed 45 marks, and was at first no more than 22 inches long, arrives therefore in the hands of the gold wire-drawers to the length of 1163520 feet; by reducing the feet to fathom, and taking the league at the rate of 2000 fathom, its length of 22 inches is changed into a length of 96 leagues and 196 fathom: An extent vastly exceeding what father Mersenne and Furetière had allowed it!

It exceeds likewise Dr. Halley's calculation; who makes the ounce of thread to be only 2680 feet.

This ingot, *long* as it is when reduced into so fine a thread, doth not rest *there*, it is still capable of being lengthened. The greatest part of our gold-thread is spun, or wound
on

on silk; and before they spin it, they *flat* it, by passing it between steel-wheels extremely polished; the wheels by *flatting* increase its length to more than a seventh part: Our ingot therefore *now* is augmented in length to more than a seventh, that is to say, we have brought it to the length of 111 leagues, *or about 300 English miles.*

But gold thus amazingly extended, how thin must it be? It appears by calculation, that its thickness doth not amount to the $\frac{1}{175000}$ of a line; and even this calculation supposes the thickness of the gold every where *equal*; which is by no means probable; for in beating the gold-leaves, whatever care they can bestow, it is impossible to beat them exactly alike: One may distinguish very perceptibly by their greater or less opacity, that they are at least as thick again in some places as in others; these leaves therefore, when they gild the ingot, must gild it *very unequally*. If we examine the thickness of the gold in those places where it is the thinnest, we shall find that it doth not exceed the 262500th part of a line; what is the 262500th part of a line! it is so amazing a smallness, that the imagination cannot grasp it.

But even yet we have not reached the utmost bounds to which the ductility of gold may be carried. Instead of *two* ounces, there might have been employed no more than *one*; in that case the gold which would

have covered the silver *laminæ*, would have had no greater thickness in certain places than the 525000th part of a line. Again, those silver *laminæ* being pressed between the flattening-wheels, and by that means the breadth enlarged to *double* what they had before; the thickness of the gold which covers the *lamina*, will be reduced to less than the millionth part of a line. The imagination stands on the verge agast, and cannot plumb her *line* so low.

The reader perhaps will be disposed to think, that the gold which covers the *laminæ* of silver hath a much greater thickness than the calculation allows it; and may conceive that the gold might be divided into small grains *separate* one from another, but yet *near enough* to give their colour to the silver; in a word, it would be natural enough to imagine that the gold which covers the *laminæ* doth not form a continued leaf: But experience *demonstrates* the contrary.

If you put *to be dissolved* in aqua-fortis some gilded wires, or some gilded plates, however small those wires, and however thin those plates may be; the aqua-fortis having dissolved the silver, the *gilded* wires and the *gilded* plates are changed into little hollow tubes, because the aqua-fortis doth not act upon gold; from whence it evidently follows, that the gold which covers the silver forms a *continued* body. Art therefore
hath

hath arrived at the power of dividing a piece of gold, of the thickness of a line, into a million of different leaves. *But gold thus yielding and ductile by human art, in return exerts a like power on the human mind; and it is difficult to say, whether it be more active or passive, a greater slave or a greater tyrant.*

Art hath not gone any thing near *so far* in working the *soft* kind of ductile bodies; amongst *these* there is scarce any but glass which we are able to extend considerably. Gums, rosins, and wax are likewise of the soft sort of ductile bodies: *From thence that elegant expression in Horace,*

Cereus in vitium flecti.

Yet wax, which is, of all the last mentioned sorts of bodies, *that* on which the manufacturing arts are the most employed, hath been but little regarded as a *ductile* body; it is true that the wax-chandlers make their wax candles pass through wire-drawing irons, but it is not to lengthen them, it is only to *round* and *polish* them.

If art *fails* in spinning out the *soft* kind of ductile bodies, yet nature hath in *these* displayed her immense power; she hath instructed an infinite number of animals to extend *such* bodies in a marvellous manner, and *we* have nothing to do but to employ the threads which those animal have prepared for us: The reader must perceive that I allude to
the

the threads of the silk-worm, yet I would not be confined to the silk of these worms, in order to shew how far nature is able to extend the soft kind of ductile bodies. The animals from which *we* derive the most advantage, are not those which *nature* hath made her choicest cabinet of treasures; it seems as if she had taken more care in forming *such* as are the most incommodious to us, and for which we have the most aversion. Spiders, for instance, are incomparably more proper than silk-worms, to shew how far nature can *lengthen* a gluish liquor.

The illustrious Malpighi, in his anatomy of silk-worms, hath described the *parts* from whence their silk is spun. *The reader will be pleased perhaps to see the account that Malpighi hath given, and therefore I will make an extract of it in the words of the author.*

“ Bombyces—fatis nutriti contabescere incipiunt, ipsorumque corporis moles parùm
 “ minuitur; per diem saltem à cibo postremò
 “ abstinēt, alvi excrementa cum sanioso
 “ quodam ichore mellei coloris ex toto ex-
 “ cernuntur, ita ut in ventre nil supersit.
 “ Externus corporis color immutatur, quæ-
 “ dam enim luciditas seu diaphaneitas cum
 “ aliquali flavitiei tinctura observatur, et
 “ sub ventre carneus luxuriat color. An-
 “ terior corporis portio, secundo et tertio
 “ excitata annulo, turgida redditur, et pur-
 pureo

“pureo colore perlucet. Eundem etiam
 “colorem referunt maculæ quædam in quinto
 “et octavo annulo insculptæ; portio pariter
 “intermedia annulorum sublutea redditur,
 “sicut et imi ventris appendices. Interius
 “ventriculus parùm depressus vitellino quo-
 “dam refertur ichore, qualem ex secto corio
 “erumpere videmus. Divagari tandem in-
 “cipiunt Bombyces, et situm pro texendo
 “folliculo aptum nanciscuntur. Tunc mu-
 “lieres sub farmentorum frustulis, in fascēs
 “recollectis, collocant, vel sub plantarum
 “exsiccatis radicibus, et truncis, in fas-
 “ciculos vel scopas compaginatīs. Lau-
 “dantur etiam ad hunc usum diversarum
 “arborum rami, ut *Castaneæ* et *Quercūs*,
 “item *Genistæ*, et consimilium. Opportu-
 “num jam locum nactus Bombyx, propriis
 “è vasculis succum, succini instar, sub
 “staminis forma effundere incipit, et ligneo
 “frustulo vel occurrenti scabritiei applicat,
 “indèque celeriter recedens, filum nere
 “perpetuat, et diverso sub situ locatis adap-
 “tat: Quod cùm glutinosum sit, aëri dum
 “exponitur, facillimè concrescit arctèque
 “hæret, et ità laxum quoddam rete, vel
 “mavis, rudes quædam contexturæ incho-
 “antur, in quarum centro, ex debita distan-
 “tia, ovalis folliculi structura et unita com-
 “pages commodè subsequi potest. Ut au-
 “tem Bombyx stamen promat, retrahit ca-
 “put, deinde attollit, et ad posteriora re-
 “curvat,

“ curvat, compressisque superioribus annulis,
 “ quasi eructat, et ad latera diverberando
 “ caput stamen exerit.

“ Inter agendum per vices caput retrahit,
 “ parùmq; hæret, deinde elongato corpore,
 “ mutatoque gressu majori cum alacritate
 “ opus aggreditur. Nec diu venit hæsitandum,
 “ an ab ore vel ab extremo alvi stamen
 “ emittat, cùm sensui manifestè pateat,
 “ sub ore brevem quandam proboscidem
 “ pendere veluti mentum, à cujus extremo
 “ perforato ex delato illuc glutinoso succo à
 “ sericis ductibus exprimitur.

After this quotation from Malpighi, I am much mistaken if the reader will not greatly relish the following lines of Vida, and be delighted to see how philosophy and poetry may blend together, and reflect light and graces on each other.

Jamq; age, jam grandes foetus, jam ducitur ætas

Ultima, turgenti filum tralucet in alvo
 Omnibus, accingunt alacres operiq; parant se.

Pabula jam saturæ fugiunt : Nova quærere regna

Ardor agit. Tollunt oculos, arrectaque terga.

Omnia vestigant latè loca, ficubi rami
 Arbutei, per quos sua possint tendere fila.

Tum

“ pureo colore perlucet. Eundem etiam
 “ colorem referunt maculæ quædam in quinto
 “ et octavo annulo insculptæ; portio pariter
 “ intermedia annulorum sublutea redditur,
 “ sicut et imi ventris appendices. Interius
 “ ventriculus parùm depressus vitellino quo-
 “ dam refertur ichore, qualem ex secto corio
 “ erumpere videmus. Divagari tandem in-
 “ cipiunt Bombyces, et situm pro texendo
 “ folliculo aptum nanciscuntur. Tunc mu-
 “ lieres sub sarmentorum frustulis, in fasces
 “ recollectis, collocant, vel sub plantarum
 “ exsiccatis radicibus, et truncis, in fas-
 “ ciculos vel scopas compaginatis. Lau-
 “ dantur etiam ad hunc usum diversarum
 “ arborum rami, ut *Castaneæ* et *Quercus*,
 “ item *Genistæ*, et consimilium. Opportu-
 “ num jam locum nactus Bombyx, propriis
 “ è vasculis succum, succini instar, sub
 “ staminis forma effundere incipit, et ligneo
 “ frustulo vel occurrenti scabritiei applicat,
 “ indèque celeriter recedens, filum nere
 “ perpetuat, et diverso sub situ locatis adap-
 “ tat: Quod cùm glutinosum sit, aëri dum
 “ exponitur, facillimè concrescit arctèque
 “ hæret, et ità laxum quoddam rete, vel
 “ mavis, rudes quædam contexturæ incho-
 “ antur, in quarum centro, ex debita distan-
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 “ tem Bombyx stamen promat, retrahit ca-
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 “ curvat,

“ curvat, compressisque superioribus annulis,
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 “ dum, an ab ore vel ab extremo alvi sta-
 “ men emittat, cùm sensui manifestè pateat,
 “ sub ore brevem quandam proboscidem
 “ pendere veluti mentum, à cujus extremo
 “ perforato ex delato illuc glutinoso succo à
 “ sericis ductibus exprimitur.

After this quotation from Malpighi, I am much mistaken if the reader will not greatly relish the following lines of Vida, and be delighted to see how philosophy and poetry may blend together, and reflect light and graces on each other.

Jamq; age, jam grandes fœtus, jam duci-
 tur ætas

Ultima, turgenti filum tralucet in alvo
 Omnibus, accingunt alacres operiq; pa-
 rant se.

Pabula jam saturæ fugiunt: Nova quærere
 regna

Ardor agit. Tollunt oculos, arrectaque
 terga.

Omnia vestigant latè loca, ficubi rami
 Arbutei, per quos sua possint tendere fila.

Tum

Tum famulæ properare, omnes provisa
parare

Sarmenta, et steriles tectis inferre genistas.
Per ramosque locant arentes agmina densa.
Summotas alias arcent, dum funditus omnes
Corporeæ excedant labes, ac, temporis
orbe

Perfecto, sua cuique dies exemeret omnem
Alvi infinceræ illuviem, purumque relin-
quat

Lanicium, et fili tralucens' simplicis au-
rum.

Sic ubi mitescunt pendentes vitibus uvæ,
Paulatim liquor ille intus rarefcit, et au-
reus

Accedit color, elucent purissima musta.

Tum demum tabulis passim sarmenta re-
lictis

Complêrunt omnes, perque atria virgea
lætæ

Exercentur, et effundunt quæsitæ per ævum
Stamina, ditis opes uteri, suspensaque den-
fos

Fila regunt inter ramos, atque ordine du-
cunt.

Mille legunt releguntque vias, atque orbi-
bus orbes

Agglomerant, cæco donec se carcere clau-
dant

Sponte sua : tanta est edendi gloria fili :
————— fila tenacia Serum

Ore vomunt saturæ.

We shall find a quite different apparatus in the body of spiders. Near the anus of the spider there are six *papillæ* or nipples.

Vid. Plate
4th, fig.
4th.

The reader will perceive here a difference from the account we have given before of spiders. Mr. Homberg mentions only four papillæ; Mr. Bon reckons five; but as Mr. de Reaumur is the latest, and I believe the most accurate writer on this and every other subject of natural history, I imagine the reader will give the preference to his authority; and I shall only observe that it is no wonder there should be such a difference on a subject of this kind, where not only a delicate hand in dissecting, and excellent microscopes, are required, but likewise great experience in the use of the microscope.

The end of each nipple is the wire-drawer, from whence issue the threads of silk; but what amazing wire-drawers! in a space less than the head of the smallest pin, there are a sufficient number of different holes to give issue to a surprising quantity of separate threads; you may *distinguish* these holes by their effects. If having chosen a large garden-spider, that is ready to lay her eggs, you apply a finger on a part of one of her nipples, as you draw back the finger, you will draw out an amazing number of separate threads. I was willing to examine their

Fig. 4th,
K. A. M.
N.

number by making use of a good microscope, I have often counted more than 70 or 80, but I saw that there were incomparably *more* that I could not count, though the threads which I had drawn had for their base only a small part of the nipple. Although I should say, that there is not the tip of a nipple but what may furnish a *thousand* threads, I might mention a number that would startle perhaps, but which appears to me too small to express the number of these threads: So that the spider having six nipples, she hath holes to give passage to *six thousand* threads.

Nature hath not confined her labour to *the piercing* holes of such an immense smallness; the threads are already formed when they arrive at the nipple, they have each their little duct or particular sheath; you find them formed and separate one from another at a considerable distance from the origin of the nipples; but in order to comprehend better all this admirable mechanism, it is necessary to lead the reader back to the very source of the liquor from which the spiders draw them.

In insects so small and soft, these delicate parts would not be easily distinguished without some little preparation: It is necessary to steep the body of the animal in boiling water, or to dry it, or to let it lie some hours in spirit of wine. After this little preparation, the *most essential* parts continue *undisturbed*, and

are sensible without the aid of the microscope. Near the origin of the belly you find two little bodies of a soft substance, (*vid. fig. 2. D D*) these are the first sources of the silk. They have pretty much the figure and transparency of a *glass drop* or *bubble*, and therefore to express ourselves more commodiously, we shall term them the *drops*. The point of each drop goes in a serpentine manner (*vid. fig. 5th, R*) and makes an infinite number of folds that tend towards the *papillæ*. From the basis of the drop their issues another branch much bigger than that which issues from its point; (*vid. S*) it makes a greater number of bendings, and moreover makes greater folds; it forms afterwards divers sorts of net-work, and takes like the other its rout towards the anus of the spider.

I have sometimes unravelled from this last branch to 9 or 10 inches in length, and yet unravelled only a part. The drops and the branches that issue from them, contain the matter *proper* for forming the silk, but it is a matter which is yet too fluid, and which in a spider that hath not been dried doth not rope into threads of any considerable length. The body of the drop is a kind of reservoir, and the two branches are a sort of emissary ducts. When you have not too much boiled the body of the spider, the branches are visibly enveloped with a mem-

L 2

brane,

brane, which hinders your seeing the transparency of the liquor. This thin membrane is rased by rubbing the duct even gently. A little nearer the anus there are two other drops somewhat smaller, these emit but one branch, which issues from the point; so that on each side of the spider there are two drops, which by three very perceptible ducts convey a liquor, and they convey it to the *proper* reservoirs, where it becomes fit for making silk.

On each side of the spider there are three bodies (*vid. fig. 2. E E*) which may be considered as the *last* reservoirs where the liquor is collected; we shall call them the grand reservoirs. (*Vid. fig. 6*). They are much bigger than the drops; the three that are ranged on the same side are placed in such manner, the one near the other, that they seem to form but one single body. The figure of each in particular is different; they have however *this* in common, *that* they have six or seven bendings, *that* through all their extent their bigness is pretty nearly the same; one of their extremities is however bigger than the other. . The biggest *V V V* is nearest to the head of the insect, and the smallest *T T T* to the anus. The three delicately fine extremities of these reservoirs terminate in a point, and are closely joined one to another like the three fingers of the middle of the hand. It is from the three

2

points

points of these reservoirs that the threads run out, they give vent to the greatest part of the threads that issue from the nipples; for each reservoir is destined to supply a nipple.

Lastly, At the origin of the nipples you may distinguish divers fleshy tubes. If you take off gently the membrane or light pellicle which appears to cover these tubes, you find that internally they are full of threads, all separate one from another, and which by consequence, under a common covering, have each its particular sheath, or are like knives in a case.

It is true, that in pursuing the rout of these threads, you find abundance of them that come from a greater distance than the point of the grand reservoirs; some appear to come from the *middle*, others from a little *lower*, others again from a little *higher*; so that the immense quantity of threads which is collected near the nipples is not intirely derived from the *points* of the reservoirs.

But how is the liquor first collected into the drops or bubbles we have mentioned? How from the drops doth it pass into the grand reservoirs? It hath probably routs that our eyes are not able to trace. The famous Malpighi, with all his penetration, when he gave the anatomy of the silk-worm, was obliged to be content with describing the vessel where the liquor is collected, from

whence the filk-worm draws the filk. He hath not explained the rout by which the liquor arrives thither, nor, *strictly speaking*, the rout by which it is discharged. What can we do in an insect much smaller than the filk-worm, and where nature hath employed six or seven thousand times *more* parts? Let us content ourselves with making some reflections on the prodigious ductility of the matter which composes their threads, on the prodigious fineness of the holes through which the threads pass, and likewise of the tubes wherein they are moulded. We have said, and we are not apprehensive of having said too much, that from the end of each nipple there may proceed more than a thousand threads. Yet the end of the nipple hath not more diameter than a small pin, and the holes are necessarily separated from each other, by intervals that must be much larger than the holes themselves. But we have hitherto considered only the full-grown spiders; if we examine the young spiders that are produced by these, we shall find that they are no sooner hatched than they begin to spin. Indeed their threads can scarce be perceived, but one sees very well the webs which are formed by them; these webs are oftentimes as thick as those of house-spiders; the reason is, because four or five hundred little spiders concur together in the same work. What must be the smallness
of

of the holes of their wire-drawers! the imagination cannot reach so far, and is scarcely able to represent to itself the smallness of each of the nipples. The *whole body* of the young spiders is perhaps less big than a *single nipple* of her that is the mother-spider: It is easy to examine that point. Each pregnant spider lays 4 or 500 eggs; these eggs are enveloped with a shell, and as soon as the little spider hath broke its shell, it begins to spin. How fine then must each of the threads be which issue from the nipples! moreover there are spiders so small at their birth, that they are not to be discerned without the aid of a microscope; they are then red, and as they are joined an infinite number of them together, they appear, to the naked eye alone, like trains of red points; yet under these almost imperceptible spiders are formed webs; they consequently spin, but of what amazing exility must the threads be which issue from the holes of their nipples! these are indeed wonderful works, but all wonder ceases and should turn into adoration, when they are considered as the works of an Almighty hand.

It must be observed that spiders spin two sorts of threads, the one serves them to weave the webs which they lay *in snare* for insects, and the other serves for enveloping their eggs: These threads differ from each other only in their greater or lesser strength; and in order

to apprehend how spiders are able to make threads more or less strong as they please, it must be considered that not only before they begin to spin they apply to some body more or less of the six nipples of the anus; but according as they apply more *forcibly*, or a *greater surface* of each of the nipples, they make threads compounded of a greater number of other threads, and by consequence much stronger and bigger.

There must be about 18 times more threads, such as issue from the wire-drawers, to compose one of the threads of the *cods*, than there are in those of the *webs*, if the quantity of threads that compose the one and the other be proportionate to their strength; for having fastened, by glue, a weight of two grains to a thread of the web, it hath commonly supported that weight without breaking, and broke when I fastened to it one of three grains; whereas the threads of the *cods* support about 36 grains, and do not break till they are charged with a greater weight.

Nature hath perhaps so greatly multiplied the number of wire-drawing holes in spiders, because the matter of the silk which is formed in their bodies, is much more *brittle* than the matter of the silk from silk-worms, and hath therefore need of being divided into *more* parts; as we find glass becomes flexible only by being divided into very fine threads.

At

At least it is ageeable to sound philosophy to conclude, that nature never *labours* in vain.

Memoirs 1713, pag. 201, and *Memoirs* 1710, pag. 400.

Explanation of plate 4th.

Figure 1st is one of the species of spiders that spin silk, seen in its upper part.

Figure 2d, the same spider seen in its under part; *AA* mark the place where the belly is joined to the breast. At *B* are the papillæ and anus. *DD* shew the place where are situated the *first* reservoirs which we have named drops or bubbles. *EE* shew in like manner the place where the *last* reservoirs are situated.

Figure 3d is a part of the belly of a spider represented larger than natural. *F* the anus. *GGGG*, the four great papillæ which are the only visible ones when the spider lays them length-wise, as they are here represented, and as she most commonly doth when you hold her in your hand.

Figure 4th is likewise a part of the belly of a spider represented bigger than nature, where you see all the papillæ disposed as they are when you press the belly of the insect. *HH* are the two papillæ most remote from the anus. *II*, two large papillæ almost equal to the precedent. *KK*, the two smallest

est papillæ. *L*, a fleshy part that covers the anus, and by a muscular power raises it upwards in order to give passage to the excrements.

OMP three papillæ represented larger than natural; different species of spiders have these papillæ made in a different manner. *O* is one of the papillæ *H*. *M* is one of the papillæ *I*; you see that from a small part of the last mentioned there issues a prodigious number of separate threads *N*. *P* is one of the papillæ of the middle *K*; they have all a great quantity of hairs, but we should have made them too confused if we had put down all the hairs that a microscope discovers.

Figure 5th, *RQS* one of the reservoirs which we have called drops; the part *Q* is the nearest to *AA* in the 2d figure. *R* is the part of the drop which terminates in a point. *S* is that which makes knots or network before it arrives at the grand reservoirs.

Figure 6th, three grand reservoirs placed near each other, but not so near as they are naturally situated. Our design was to shew that they are *three*, in the animal they seem, at first view, to make but *one* body. *V* the ends nearest the head of the insect. *TTT* the ends most tapering and the nearest to the anus.

Figure 7th, one of these reservoirs seen separately.

Of

Fig. 1.



Fig. 2.



Fig. 3.

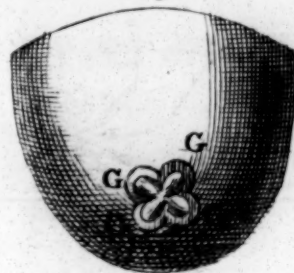


Fig. 4.

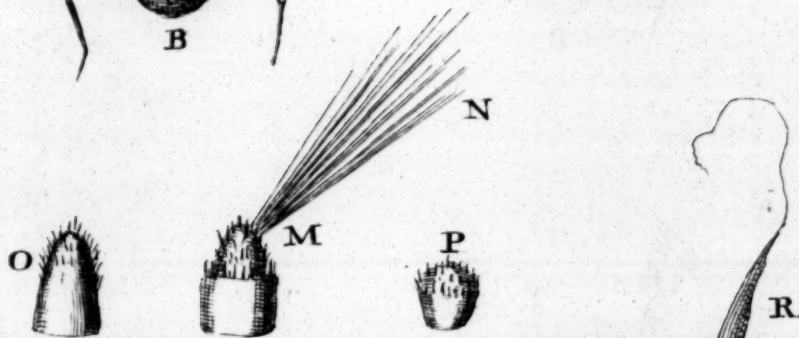
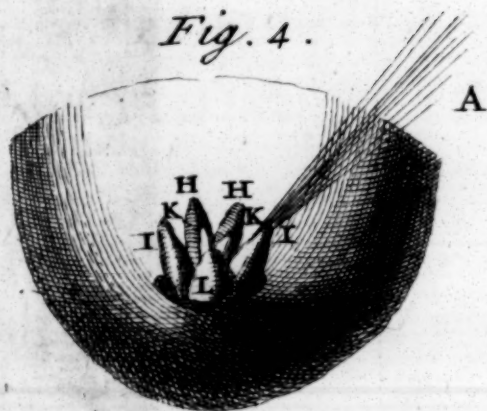


Fig. 5.

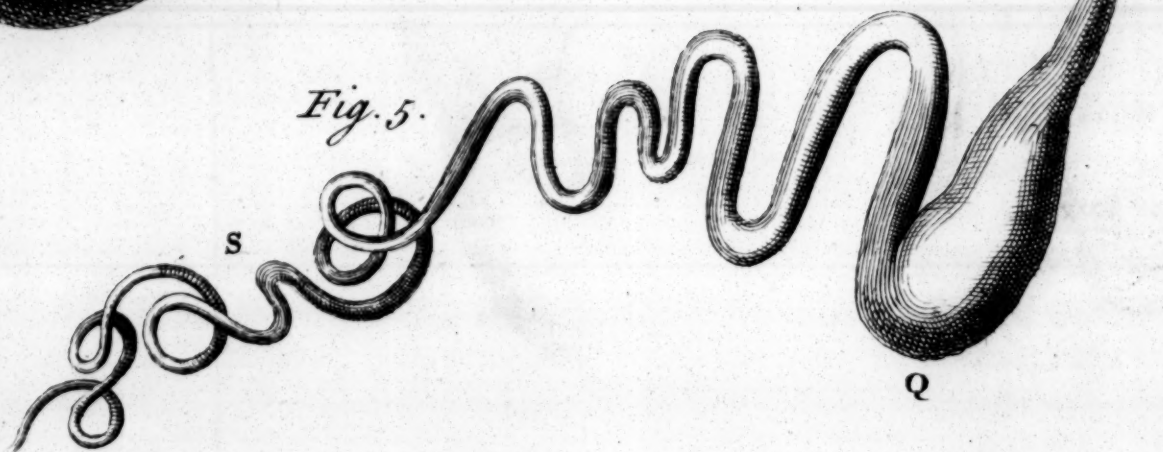
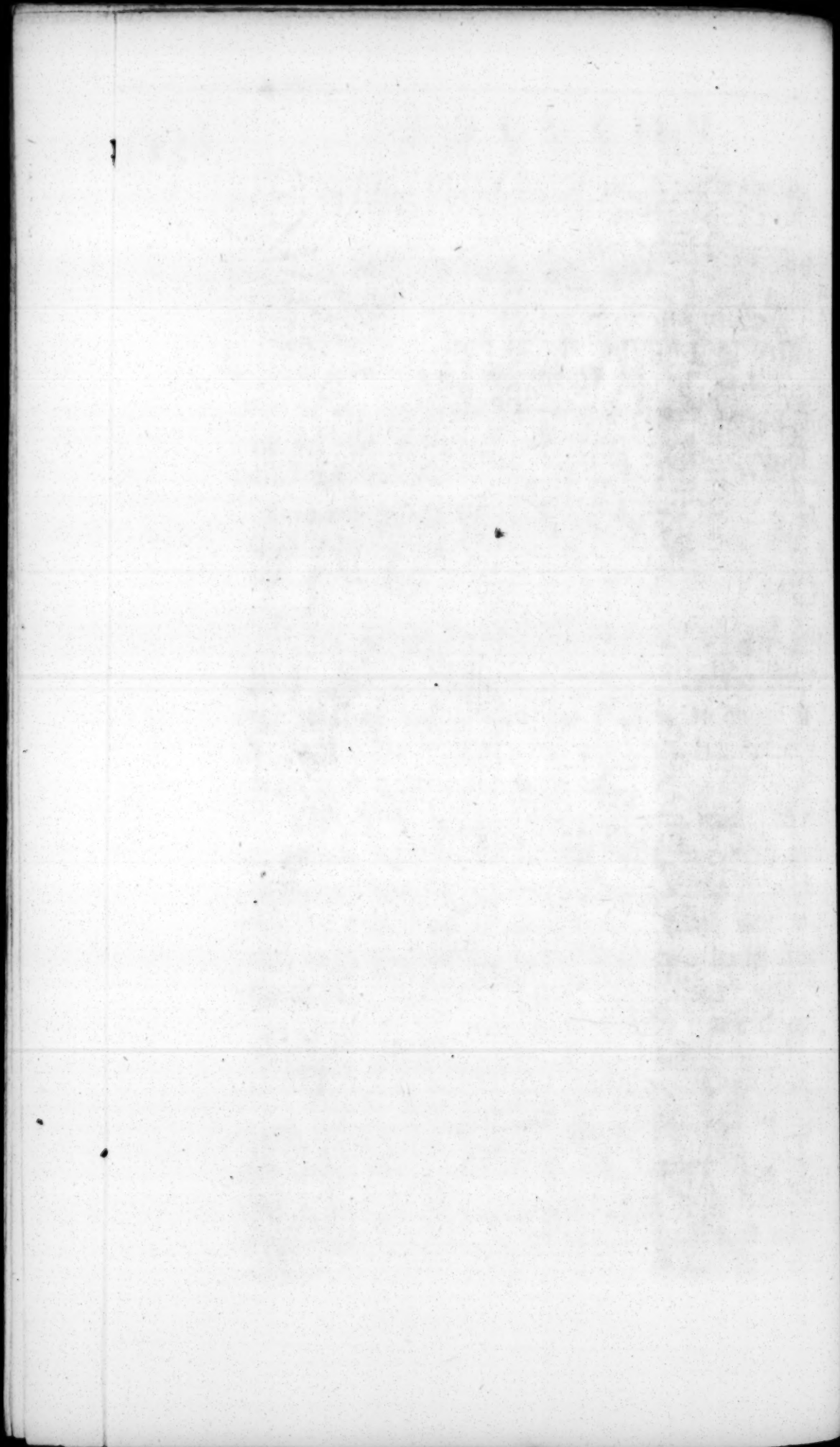


Fig. 6.



Fig. 7.





Of all the curiosities of natural history the labour of the bees is one of those that cause the greatest admiration. Yet though these marvellous insects deserve a great attention, they are not the only insects that display their industry in building hives; there are many others that have the same artifice: But the little benefit that *we* derive from the labour of some of these animals hath been the cause of their being neglected.

Observations on gum-lacca, and other animal substances which furnish the purple dye, by Mr. Geo. Froy the younger.

Amongst the *useful* animals we may reckon those insects that produce, in the Indies, the gum lacca; which hath been always in repute for dying and for other uses; yet the manner of its production hath never been satisfactorily cleared up, either for want of observers on the spot, or because the insects that *labour* it, not being brought up with a particular care, like our bees, could not be examined with sufficient exactness.

I shall therefore join my observations on this matter to the few lights that authors have given us of it, in order to make known the nature of gum-lacca much better than it hath hitherto been.

The name of *gum* doth not seem suitable to it, since it is rather a sort of wax, as I shall shew in the course of this memoir.

The name of lac or loc is derived to it from the Arabians, from whom the Indians received the term. They name it likewise

trec

trec in the kingdom of Pegu and of Martaban.

The first authors that have treated of the lacca, have told us that it grows in the Indies, and particularly in the kingdom of Pegu.

As there is a fondness of believing that the ancients were ignorant of nothing, several have imagined that it was known by Dioscorides and Scrapion, and that it is what the first hath named *concamum*; but the description which these authors have left us of it, is too imperfect to be able to form any judgment.

The principal sort of lacca, and which hath given occasion to my observations, is what they call lacca on sticks, because it is brought to us fastened to little branches on which it hath been formed.

The first thing that presents itself to our examination, is to know whether this gum is not an *exudation* from those little branches where you find it attached. This notion is not very probable, because in breaking it and detaching it from those little sticks you see no *emissary* by which it could have flowed. Besides, as this gum is very abundant, and the sticks are often very little, it is evident that it is not produced from thence: This is likewise the sentiment of father Tachard, who says, that in an account which had been transmitted to him, when they make an incision in these sorts of trees there issues indeed

deed a gum, but that it is of a nature quite different from the lacca.

It is known *in general* to be the work of a sort of insect; some say, that they are flying ants who deposite this matter on the slender branches of a tree called *ber*. Others will have them to be common ants, and others again, that they are flies. There are even some who say, that the sticks of the lacca are nothing else than boughs, which the inhabitants take care to plant in the ground in great abundance, in order to serve as props for the work of these little insects. As to the nature of this work they give us no certain light.

It hath appeared to me, after a careful examination, that it could be nothing but a sort of hive resembling in some measure the *natural* hives of bees and other insects. In effect, when you break it, you find it divided into several cells or apartments, of a very uniform figure, and which shews that it was never a gum or flowing rosin from trees.

Each of these cells is oblong, with several flat fronts, sometimes intirely round, according as the matter, being yet soft, hath been disranged, and hath flowed round the branch that supports it.

This disorder is the cause that these cells have some difference in their construction, yet they are most commonly *swain* in the middle, ending in a sharp point on that side

side where they touch the branch, and terminated on the other by a point more or less rounded and pierced with a little hole, such as are the holes with which the whole surface of the lacca appears bored, when it is looked at attentively.

You commonly see at the extremities of the cells two little white lines, of which it is difficult to determine either the matter or the use.

The partition-walls of these *cells* are extremely fine, and intirely resembling those of bee-hives; but as *they* have nothing to defend them from the injuries of the air, they are covered by a *lay* of the same sort of wax very compact and very thick, which serves them for a shelter.

From hence we may conjecture, that these animals do not work with less industry than the bee, seeing they have much fewer conveniences.

It is certain that these cells are made for lodging something, and that it is not a mere excrement which these insects deposite, as some have imagined; moreover you discover in them little bodies more or less puffed up, and which are moulded in them. The first observers took them for the wings or other parts of those ants to whom they attribute the lacca, and supposed that they intombed themselves there. These little bodies are of a beautiful red, some more deep and others less;

less; and when you crush them, they fall into a powder of as beautiful a colour as the cochineal.

Now it is not probable that they are the *parts* of those animals that form the lacca; at least one cannot sufficiently *distinguish* them: And with what design should they make themselves such a tomb?

It is more reasonable to think, that these *cells* are destined for their swarms like *those* of bees, and that the *little bodies* are the embryos of insects not yet produced, or the *exuviae* of those that have already sprung

————— *in luminis oras* ;

as we observe in the kermes, which is sometimes called the *scarlet grain*, in the gall-nut, and the other excrescences which proceed from the bite of insects, and of which I shall have occasion to speak in order to clear up the better this subject.

The little bodies which we have mentioned are oblong, wrinkled or shagreened like cochineal, terminated at one end by a *point*, and at the other by *two*, and sometimes by a *third*. By putting them in water they swell like the cochineal, tinge it with as beautiful a colour, and assume a like figure; infomuch, that the inspection alone gives a suspicion that they are little bodies of insects in some state or other. These give the lacca the red tincture that it appears to have, for when it is absolutely destitute of them, or but little furnished

furnished with them, you scarce see a very slight tincture.

It appears therefore that the lacca is but a sort of wax, which forms, as I may say, the body of the hive.

This wax affords an agreeable smell when burnt, but with regard to the *little bodies* which are included in the cells, *they* throw out by burning an offensive odour like animal substances.

Several of these little bodies, which for the most part are hollow, are found sometimes intirely rotten, and full of mouldiness, others are full of a powder wherein you discover with the assistance of a microscope abundance of long transparent insects with several feet, of which it is not easy to determine the species. Perhaps they are the insects which have been deposited there by those that form the lacca; or perhaps they are other insects occasionally produced there; for you find different sorts which penetrate the whole substance of the lacca, and which lodge and deposite their eggs there in abundance, as it happens sometimes to beehives:

———*nam sæpe favos ignotus adedit
Stellio, et lucifugis congesta cubilia blattis.*

All these observations incline me to believe, that what is called gum-lac is not, precisely speaking, of the genus of gums or rosins,
but

but a sort of wax gathered and kneaded by certain insects ; and that the white streaks, which I have observed in the cells, may be the remains of a matter proper for nourishing the little insects as soon as they begin to be hatched.

But what confirms me more in this opinion, that the *lacca* is a true hive, is the examination that I have made of *what* comes from the island of Madagascar, for *that* doth not differ scarce at all from common wax, it hath the colour and the odour ; and is formed, like *that* from the Indies, round branches of trees.

Yet I mean not to destroy intirely the opinion of those who maintain, that there is a gum-lacca which exudes from the leaves of certain trees ; since it is possible that the ants, who, according to the account of father Tachard, gather it commonly upon different flowers, might find it likewise in abundance upon some trees, from whence one might collect it without their aid. But then this sort of lacca affords not the red tincture, which is derived solely from the chrysalid *exuviae* that are lodged in the cells.

Ray.

The analyfis of the lacca serves likewise to confirm the comparifon I have made of it with the common bees-wax ; for one draws from it pretty near the same *principles*, that is to say, an acid spirit and a butter, as Mr. Lemery hath remarked. But as the *animal*

M

parts

parts contained in the lacca must furnish something particular, and as it is not easy to make the analysis of *them* separately, I was obliged, in order to examine this affair, to take a sort of lacca, which after having been made use of in dying, was deprived absolutely of all that it could contain of *animal*, and I remarked by the analysis between this lacca, which was in grains, and the lacca on sticks, a difference which justifies my conjectures on the little animals included in the cells; for the acid spirit that is drawn from the latter is mixed with a volatile spirit, which the animal part alone is capable of furnishing; this is easily discoverable by the *white precipitation* that results from its mixture with the solution of corrosive sublimate; *which* is not effected by the acid spirit drawn from the *lacca in grains*, because *it* doth not retain any animal parts.

There is a curious observation by Dr. Wall, in the philosophical transactions, on the luminous quality of the lacca, which I beg leave to cite.

“ I must not forget to speak of another
 “ substance, not hitherto by any (as I know)
 “ taken notice of to be endued with a lumi-
 “ nous quality, which is also another natu-
 “ ral *phosphorus* or *noctiluca*, and that is gum
 “ *lac*, and also red sealing-wax, which is
 “ made with *gum lac* and *cinnabar*, the *cin-*
 “ *nabar* no way impeding but rather pro-
 “ moting

“ moting its luminous quality ; by all the
“ trials I have hitherto made of *lac* and seal-
“ ing-wax, I find that though the crack-
“ lings upon friction are as plentiful in the
“ day time, as when the sun is down, yet in
“ the darkest places I could discover but a
“ little appearance of light, so that this de-
“ serves the name of a *noctiluca*, or phos-
“ phorus, it being no other than a vegetable
“ *oleosum* coagulated with an animal volatile
“ *acidum*. I don’t know in the animal king-
“ dom any thing but pismires, that affords a
“ volatile acid ; and in the East-Indies there
“ is a large kind of them, that live on the
“ sap of certain plants, affording both a gum
“ and a colour, which sap passing through
“ the body of those insects or animals, is by
“ their acid spirit converted into an animal
“ nature ; which is the reason, that with the
“ colour extracted from *gum-lac* (which *gum-*
“ *lac* is nothing else but the excrements of
“ these insects or animals) almost as good,
“ and full as lasting, colours are made as
“ from the cochineal : I am the more con-
“ firmed in this, because I know of an ar-
“ tificial way of converting vegetable colours
“ into an animal nature, very much like
“ this, by which the colours are made more
“ pleasant and permanent. After the same
“ manner the remaining gum, which is an
“ *oleosum*, being digested and passing through
“ the bodies of those insects or animals, is

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“ *oleosum*, being digested and passing through
“ the bodies of those insects or animals, is

“ by their volatile acid converted into a vegetable animal *phosphorus* or *noctiluca*.

Vid. Jones Abridgement, Vol. 4. Part 2.
Page 278.

The difference between Dr. Wall and Mr. Geoffroy consists therefore chiefly in this, that the former considers the volatile principle of the lacca to arise from the excrements of the animal, or from having passed through its body; the latter imputes the same animal principle to the chrysalides or nymphæ that are lodged in the cells.

In observing the parts of animals that were contained in the two sorts of lacca, I have been obliged to compare them with the other animal substances from which the purple tincture is derived.

Cf the
kermes.

One of those that hath been a long time in use is the kermes, which I may range in the number of galls, on account of its being an excrescence produced on a tree by the bite of an insect. *Kermes* is an Arabic word which signifies a little worm, and tho' it cannot be doubted, but the *kermes* was known by the ancients, and in use amongst them, yet we have a confused notion, that there hath been abundance of different substances, which have all served for the scarlet dye, and which grow in divers countries, and on several sorts of trees or plants: But the kermes alone

alone of all those substances still continues to be employed a little by our dyers.

The kermes are little bladders swollen and round like pease which are fastened upon the *ilex, aculeata cocci glandifera*, C. B. P. 425. There is observed in the spring upon the leaves and young shoots of this shrub a little bladder, which at first is no bigger than a millet grain; it is caused by the bite of an insect that deposits there her eggs, it grows bigger by degrees, and becomes of a grey ash-colour on its surface; this colour is nothing but a kind of bloom, such as we see on fruits, and which conceals the red colour. The Greeks have named it Κόκκος Βαφικὸς, the Arabians *kermes alkermes*, the Latins *coccum* or *vermiculus*, from whence comes our French term *vermillon*. The name of *scarlet* which is most in use is thought to be derived from the ancient Celtic.

When this *gall* is perfectly ripe, they gather it, and squeeze from it the juice or pulp, or at least wash it with vinegar to kill the insects that are included within. Without this precaution, these little animals as soon as they are hatched, would leave the husks empty. They fall forth like so many little flies. After having well examined these husks, when dry, I observed that there were a great many of them *empty*, and that *these* had an hole at the *place* by *which* they adhere to the leaf of the tree.

In order to get a more complete knowledge of the kermes, I have made an analysis of it.

Besides some phlegm which had the smell of the kermes, but which yielded nothing, I have drawn from it several portions of urinous and volatile liquors, that have not indeed altered the colour of the *turnesol*, but which have turned *green* the syrups of violets and roses. From a pound of kermes which I had put into the retort, I have drawn upwards of half an ounce of a beautiful volatile salt crySTALLISED, mixed with a citron-coloured oil. The fœtid oil was very abundant, but not black, it was of a deep yellow, and as thick as butter.

The kermes therefore is a substance that belongs more to the animal than to the vegetable kingdom; since it furnishes a great quantity of volatile salt, and of a pure sulphur.

Besides the above-mentioned kermes there is another which is found attached to the roots of a plant, and which is called coccus polonicus: the plant on which it is most commonly found is the polygonum cocciferum C. B. Mr. Brey-nius published in 1731 at Dantzick observations on this species of kermes, and hath proved incontestably, that it is an insect.

*Vid. Reaumur Histoire des insect, Vol. 4.
P. 111.*

The

The cochineal, on account of the beauty ^{Of the} of its colour, and the great quantity that is ^{cochineal.} brought from America, hath rendered almost useless all the other substances that were made use of before.

I have thought it proper therefore to enter into a detail on the nature of cochineal. As common as it is, authors that have treated of it, have been greatly *divided* on this subject. Some have said that it was an insect, and others on the contrary have maintained it to be a feed.

This diversity of opinions is observable in the first authors that have treated of it. It is true, that father Plumier, a skilful botanist, hath lately given great weight to the opinion that the cochineal, which is called *mesteque*, is nothing else but a little animal resembling a bug, which they collect on a plant of America, named *Opuntia major validissimis aculeis munita*, inst. R. H. and which we call the Indian fig. But on the other side, Pomet in his history of drugs, pretended that this father was mistaken, and cited for a proof, the letters of one of his friends, who was actually on the spot, and who assured him, that the cochineal was a grain.

Altho' this opinion hath not been since much followed, yet I do not know of any one that hath expressly refuted it, and removed the doubt, which this contrariety might leave.

One would think that the inspection alone of cochineal might be sufficient to determine the dispute; but being dried as they bring it to us, you discover in it nothing that can make you incline more to the one opinion than to the other.

Being willing therefore to be better informed, I put some in water, where it swelled and resumed a suppleness and a shape that can belong to nothing but an insect. You may distinguish there by the *sight alone* the head, the anus, and a sort of rings; and with the assistance of a microscope, you may perceive the legs. I have prepared some in such a manner, that one could see the articulations of the legs, which were three on each side; but I could not discern any marks of wings, as some authors have pretended, who would have the cochineal to be a sort of *beetle*. Having opened some of the biggest, there issued a red liquor together with a sort of cluster that is fastened to an hollow tube, and which is furnished with little grains that seen through the microscope appear like little cochineals.

This configuration as well internal as external of the parts of the cochineal, shews sufficiently that it is truly an insect, as father Plumier hath observed, who hath very properly compared it to a bug; it is not, however, quite so flat, and I imagine that it still more resembles, on account of its roundness,

those sorts of insects that attach themselves to dogs.

What is surprising in the cochineal, is that it receives the beautiful red, which it furnishes, upon leaves that are of a pale green; for father Plumier remarks, that these insects are produced indeed on divers sorts of trees, but that they do not afford this beautiful scarlet colour, unless they have been brought up on the leaves of the *opuntia*. How are these *leaves* capable of giving a colour which they have not themselves? In order to resolve this question, it is proper to observe, that the Indian fig, which is the fruit of the *opuntia*, is of a red colour, so strong that it even tinges the urine of those who eat it; which hath sometimes frightened those who not knowing the cause have imagined, that they voided bloody urine: It may therefore be conjectured, that the substance of this plant undergoes in passing through the body of the insect the same *alteration* as *that* which causes the like colour in the fruit.

If the observations which I have been relating are not sufficient to prove, that the cochineal is an insect, yet the *analysis* I have made of it, would *alone* sufficiently demonstrate it; for you draw from it precisely the same principles as the parts of animals usually furnish, for instance, a *phlegm* of the same odour as *that* which arises from the distillation of harts-horn, a spirit and volatile salt

salt pretty nearly in the same proportion as you draw from some insects, such as the millepedes, and a fetid oil of a deep *red* and very thick.

Memoirs 1714, pag. 121.

“ Mr. Ruuffcher printed a book in the
 “ year 1729 at Amsterdam, which is suffi-
 “ cient to demonstrate, that the cochineal is
 “ an insect to those who are the most preju-
 “ diced with the contrary opinion. This is
 “ perhaps the first instance that a question of
 “ natural history hath been handled and de-
 “ cided by a court of judicature. The na-
 “ ture of the cochineal is established in that
 “ book, by depositions in form, taken upon
 “ oath before a judge in the city of *Antique-*
 “ *ra*, situated in the valley of *Oaxaca*, a very
 “ extensive province of Mexico, where the co-
 “ chineal is gathered. These depositions are
 “ of people of the country who have seen the
 “ cochineal reared, and have likewise reared
 “ those insects themselves. A dispute that
 “ Mr. De Ruuffcher had with one of his
 “ acquaintance on the nature of the cochi-
 “ neal, hath procured us this information.
 “ His friend maintained that it was not an
 “ insect, but that it was a fruit. The dis-
 “ pute grew warm, the two friends laid a
 “ wager; at last the dispute became a trial
 “ which was decided by arbitrators chosen
 “ by the two parties, after having sent for
 “ proofs

“ proofs authentic and necessary for the deter-
 “ mination. All the witnesses depose una-
 “ nimously, that the cochineal is an insect,
 “ and a viviparous insect; which passes a
 “ great part of its life fixed on a plant, from
 “ whence it sucks the juice, and where it
 “ lays its young. One of the depositions a-
 “ vers, that this insect is not subject to any
 “ metamorphosis.

Vid. Mr. De Reaumur's *Histoire des insectes*,
 Vol. 4. pag. 89.

The kingdom of France is not rich in
 mines of jewels, but its excellent soil sup-
 plies abundantly those commodities, whose
 value is independent of opinion. It is not
 however intirely destitute of such rare stones,
 as an almost unanimous consent hath placed
 in high esteem; but *we* are not always suffi-
 ciently attentive to make advantage of our
 own native riches. Persia is famous amongst
 us, as in the rest of the world, for its tur-
 coises; perhaps we envy her this commodi-
 ty, whilst we are ignorant that the *mines* of
 these stones are more rare in Persia than in
 France, and that the turcoises which we ne-
 glect to draw from *our own*, are not much in-
 ferior to those which come from the east, to
 speak no higher in their praise for the pre-
 sent; we shall consider that point, when, af-
 ter having examined turcoises in general, we
 shall come to a particular examination of those

Of the
 turcois
 stone, by
 Mr. de
 Reaumur.

in this kingdom. It is a subject that deserves the attention of such as love natural history and philosophy.

The turcois is considered as the *first* of the opake stones; its colour is blue. The blue of those which are the most esteemed is neither *deep* nor *clear*; above all it ought not to be whitish, or in the jewellers term, it ought not to resemble the blue of starch: It ought rather to approach to the blue of verdgrease in lump; without having any sensible tint of green, it may incline a little to the greenish.

Of the precious stones it is one of the softest; its hardness is scarcely equal to that of crystals, or transparent flints; but there are some much more soft than others: The hardest are, *cæteris paribus*, the most beautiful, and for this reason, because the vivacity of the polish is in all stones proportionate to their hardness. Those that are of a beautiful colour, of a lively polish, that have not on their surface either threads, streaks, or inequalities, and which weigh several carats, are very dear; Rosnel, a famous jeweller and author of a treatise on precious stones, estimates such as we have been describing, and which are very rare, of equal value with the diamond.

It is probable, that the first turcoises which were seen in Europe, were brought from Turkey, and that from thence their name is derived. It is not easy to decide under what
name

name the ancients have spoke of them; they have characterised the greatest part of stones in so loose a manner, that it is often impossible to know them again.

Some imagine that this stone is that which Pliny names *borea*, and which he hath placed amongst the different species of jaspers. Others will have it to be what he hath called *callais*, though he hath said expressly that this last stone is green. *The words of Pliny are, è viridi pallens.*

Rosnel tells us the way they get the turcoises stones from their mines according to the history, or rather according to the tale, that Pliny hath related of the method in which they get the *callais*. Rosnel says, that the turcoises are found only on the summit of some rocks, which the ice renders inaccessible, that by stones from slings they knock them off, and that from thence it happens that few are found intire. Thus Pliny speaks of the *callais*, *in rupibus inviis et gelidis, oculi figura extuberans, leviterque adhærens, nec ut agnata petris, sed ut apposita. Quamobrem scandere ad eam pigritia pedum equestres populos tædet, simul et periculum terret. Ergo fundis è longinquo incessunt, et cum toto musco excutiunt. Hoc vectigal, hoc est gestamen in cervice ac digitis gratissimum. Hic census, hæc gloria à pueritia dejectorum numerum prædicantium, in quo varia fortuna.*

Many

Many things extremely uncertain have been given out with regard to the country where the turcoises are found. It hath been pretended that they have been found in several parts of the East-Indies, and that those were the most beautiful. Boëtius * adds, that Spain produces them as well as Germany, and that they are to be met with in Bohemia, and in Silesia.

Tavernier, who was engaged by his trade to inform himself of jewels, and who had no mercy on his legs, assures us that there are in the east but two mines of turcoises known; that they are both in Persia; "The
 " one, says he, which is called the old rock, is
 " three days journey from Meched towards
 " the north-west, near a large town named
 " Necabourg. The other, which is called
 " the new rock, is distant five days journey
 " from Meched. Those of the new rock
 " are of a bad blue, inclining to a white,
 " and little esteemed. The king of Persia
 " hath, for several years, forbid to rum-
 " mage the old-rock for any one but him-
 " self."

There is reason to think, that the old-rock of Persia is exhausted, or at least that the stones from thence are much rarer than in the time of Tavernier.

Many must remember the embassy which the king of Persia sent to Lewis XIV; it is well

* De Boodt.

well known that abundance of turcoises stones made a part of the presents that were brought from those distant countries: Yet all those turcoises are of the new-rock; their colour inclines to a white, like those Tavernier speaks of; they are not fit to take a beautiful polish, and their bigness is not considerable. In a word, it would not perhaps be difficult for us to send into Persia more beautiful turcoises and much bigger, if we would search into our own mines, to draw from thence the stones which they contain.

Jewellers and lapidaries divide turcoises as they do all other precious stones into oriental and occidental, or still more commonly into turcoises of the old-rock and turcoises of the new-rock. I ordered a skilful jeweller to cut several pieces of turcoises which had been certainly taken from our own mines. I wanted to know what hardness they had, what polish they would take upon the wheel, and what would be their colour after they had been polished. The lapidary found a great difference between the pieces, and indeed there was a very great one: As he cut them, he shewed me those that were of the old, and those that were of the new-rock. Among those that he called of the old-rock, there was one, that was indeed small, but which was not inferior in hardness to any of the stones of this kind; its polish by consequence

sequence was extremely lively, and its colour perfectly beautiful. It signified nothing to tell the lapidary that these different pieces had come from the same mine; he did not contest the fact, but yet did not change his language; and *that* for this reason, because a perfect stone in its kind, or a stone of the old-rock, are with them synonymous expressions.

The mines of this kingdom which furnish the turcoises are in the lower Languedoc, near the town of Simore and in the neighbourhood, as at Baillabatz and at Laymont: There are some likewise bordering on this country towards Auch, at Gimont and at Castres.

One finds several of these mines within the jurisdiction of Simore: Chance hath always had a great share in the discovery of mines, but in this instance it might have taught us likewise the materials that compose these stones. All other precious stones are drawn from their mine with the colour which we see them have when they are cut. We can add nothing to their colour, but we may weaken the colour of some sorts of stones: With the aid of fire, for example, we may make more pale the too deep colour of a sapphire; by the same means you may take away entirely the colour from a sapphire that is pale, and bring it to the water of a diamond. Our turcoises on the contrary are naturally whitish or yellowish, but if you expose them for
some

some time to the action of fire, instead of becoming more white they take a blue colour.

However surprising it may be at first view, yet there is scarce any room to doubt but that the materials of our turcoises are petrified bones. The greatest part of the pieces which have been taken from mines, have had the external figure of bones. Amongst the specimens from the mine at Simore, which have been sent me, by the orders of his royal highness the duke of Orleans, I have found some that are not less visibly teeth than the *glossopetræ*. They have in like manner all their enamel, which hath been perfectly preserved; but the bony part, that which the enamel covered, and likewise that which made the root of the tooth, and was never covered with enamel, is a white stone, which put to the fire becomes a turcois, by taking the blue colour.

If the external figure of the pieces from the turcoise mine is not sufficient to prove that these stones are petrified bones; yet the examination of their substance will afford a stronger proof. At the first view this stone appears different from others; it seems to have something of the ivory or of an osseous matter; it hath a polish of a middle kind between that of opaque flints, and that of bones or ivory. Notwithstanding this polish it fastens like the *boles* to the tongue, when put

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upon it. Considered more attentively, you discover that its composition is in *strata* or scales; yet this is not a difference that *characterises* it, it is a structure common to bones and to abundance of stones to be formed in *leaves*: But there is one thing *particular* to the turcoises, which is, that these leaves have only served, as I may say, to form the mould into which the stony matter hath insinuated itself. The softer and more imperfect the stones are, the more distinguishable are the different directions of the fibres and *laminae*, when you break them; you discern the *intersections* of fibres such as they would appear in bones *broken* after the same manner, and which certainly do not belong to any sort of stone; insomuch, that I think I can perceive the very traces of cells of bones ranged in their natural order.

The turcoises, at least those of France, are not naturally blue; it is the fire that gives them that colour. Before the fire is applied to them, you see them besprinkled through their whole substance with spots, or veins, or little circles, that are of a dark-blue colour: These are the reservoirs of a blue matter, whose blue is extremely deep, because it is collected in heaps; and the fire, by rarefying, diffuses it more equally through the whole stone.

But of what animals are these the bones? This is a question that I cannot yet resolve, but

but time perhaps will discover, in like manner, as we have found out the fish from which the glossopetræ or pretended tongues of serpents are produced*: It is probable that the teeth I mentioned above are of some sea animals, for I know of no terrestrial animals that have the like.

The coloured matter that fills the reservoirs, which I take to have been originally cells of the bones, and that tinges afterwards the whole stone, is without doubt a mineral matter; but is it a simple mineral matter, such as the cobalt or the matter with which they make the *ultramarine*, and the zaffre, which give the most beautiful blue to china and to earthen-ware, or is it a metalline matter? This is an enquiry on which I have not been able to satisfy myself; but it hath appeared to me that what tinges *our* turcoises is different from what tinges the *Persian*.

I have found it possible to take away the *tincture* of our turcoises, in the same manner as one takes away *that* of coral: Of all the dissolvents which I have tried, distilled vinegar hath succeeded the best. If you put a pretty thick piece of turcois to soak in this vinegar, in an hour or two its angles become white, and in two or three days time all the upper part of the stone, and even almost all

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its

* *Viz. from the teeth of the carcharias or white shark.*
Vid. History 1705, pag. 35.

its internal part, assumes the same colour. The vinegar, in taking away the colour, dissolves likewise a little the stone: It is always covered with a sort of white cream, composed of the parts that have been detached. Lemon juice dissolves likewise these sorts of stones, but it only weakens their colour, and what is found underneath that kind of cream which we just mentioned is blue.

As to *aqua-fortis* and *aqua regia*, they are not proper to draw off the tincture from our turcoises, they dissolve too quick the whole substance of the stone: But they give us a method of distinguishing the turcoises of Persia from those of this kingdom. The *aqua-fortis* doth not act upon those of Persia; from whence it follows that these two sorts of stones, though alike in appearance, are nevertheless of a very different nature: It would be wrong however to draw from thence a consequence disadvantageous to ours, *namely* to think them softer. *Aqua-fortis*, that acts efficaciously on iron, hath no power at all upon wax.

The *aqua regia* acts likewise differently on these two sorts of stones; it dissolves entirely ours, but it reduces the Persian into a sort of paste more whitish than the turcois was, but which is not intirely deprived of all its blue colour. May not gold enter into the tincture of the Persian turcoises? At least it follows that there is in it a matter on which
the

the *aqua-regia* hath hold, but at the same time this *dissolvent* must act on these turcoises as it acts on a mass of metal compounded of a mixture of gold and silver.

If the blue of ours was more durable than *that* of the Persian turcoises, as Berquen affirms, it would be one of their advantages; but this is a matter not easy to be ascertained by experiments, it requires a long series of years to determine it: I think however that those of Persia have a greater disposition to become green; for whilst the blue of ours whitens in the distilled vinegar, the blue of the Persian turns green in it.

In general these sorts of stones have a singular defect, which is, that without the aid of any other dissolvent than time, their colour changes. Insensibly their blue takes tints inclining to a green, they become greenish, and at last intirely green; whereas the original colours of all other precious stones for ever blaze the same.

Memoirs 1715, pag. 174.

Dr. Mortimer mentions in the Philosophical Transactions, N°. 482, pag. 432. that Sir Hans Sloane, in his noble museum has several specimens of the oriental turcoises, all botryoid; especially a mass from China, about three inches long, two broad, and near an inch thick: All which seem to be copper ores: The doctor thinks that the botryoid form is a demonstration that it is

the product of fire, which had once melted the substance; and that when it cooled, its surface was formed into bubbles and blisters, in the same manner as the hæmatitis botryoïdes or bloodstone; whose surface consists of knobs, resembling a bunch of grapes. Sir Hans has likewise samples of turcoises from Spain, and the south of France; which are all small, and seem really to be pieces of ivory tinged with copper. Vid. Philosophical Transact.

I must not omit mentioning a fact which seems greatly to strengthen the opinion of Mr. de Reaumur.

“ The Royal Academy of Polite Literature, Sciences, and Arts, at Bourdeaux
 “ hath sent to his royal highness the Regent,
 “ who hath had the goodness to communicate them to this Academy, some bones
 “ that were found in a rock, together with
 “ a memoir of which we shall only give an
 “ extract. In the parish of Haux, lying
 “ between two seas, at half a league distance
 “ from the port of Langoiran, a point of a
 “ rock 11 feet high was detached from a
 “ hill, which was before near 30 feet
 “ high, and by its fall it dispersed in the
 “ vale a great quantity of bones or fragments of bones; some of which are petrified. They are undoubtedly bones, but
 “ it is very difficult to determine to what
 “ animals they belong. The greatest number
 “ ber

“ ber of them are teeth, some perhaps of an
“ ox or horse, but the greatest part are too
“ big for those species of animals, not to
“ mention the difference of shape. There
“ are thigh bones, and even a fragment of
“ stags or elks antlers. The whole was
“ enveloped with common earth, and in-
“ cluded between two beds of rock. One
“ must necessarily conceive, that the car-
“ cases of animals having been thrown into
“ an hollow rock, and the flesh putrefied,
“ there was formed above this pile of bones
“ a rock 11 feet high ; which must require
“ a long series of ages.

“ The gentlemen of the Academy at
“ Bourdeaux, who have examined this mat-
“ ter, like skilful philosophers, were willing
“ to try on these bones what Mr. de Reau-
“ mur had said of the origin of turcoises.
“ And they found, in reality, that a great
“ number of fragments put upon a very brisk
“ fire, became of a beautiful turcoise blue,
“ that some small pieces assumed the consist-
“ ance of those stones, and that cut by a
“ lapidary they received the polish. The
“ gentlemen carried their curiosity still fur-
“ ther, they made experiments on recent
“ bones, which only turned black, except
“ some little pieces which inclined to a blue.
“ From thence they conclude with great
“ probability, that bones in order to be-
“ come turcoises require a very long con-
“ tinuance

“ tinuance in the ground, and that the
 “ same matter, which makes the black in
 “ recent bones, makes the blue in such as
 “ have been long interred, because it ac-
 “ quires in the ground by very slow degrees
 “ the due maturity. It must not be forgot
 “ that bones, which belonged visibly to dif-
 “ ferent animals, succeeded equally well in
 “ becoming turcoises.

History 1719, pag. 23.

A curious
 observati-
 on on the
 Brazile
 topaz.

It hath been long known that the oriental precious stones can endure a very violent force of fire, without their colour being altered by it; and that on the contrary the occidental lose by this means in a very little time their colour, and become like crystal if they are transparent, or of an unpolished white if they are opaque: But it was not known that the topaz of Brazile could not be comprehended in either of the two kinds we have been mentioning; it hath the singular property of quitting by the fire its yellow colour, and assuming a rose colour resembling a balais Ruby, and so much the more lively as the yellow of the stone was deeper.

The process is very simple, you need only place the topaz in a crucible filled with ashes, to increase the fire by degrees, till the crucible is red hot, and after having kept it some time in that state to let the fire go out: When the whole is grown cold, you will find

find the topaz converted into a true balas ruby; we say *converted*, because it is not possible to perceive the least difference between the natural balas rubies and these. Which has induced several jewellers, who knew the secret, to make a mystery of it; and it is to Mr. Dumelle, a goldsmith and stone setter, who communicated it to Mr. Guettard, that the Academy is obliged for this intelligence. He was willing on this occasion to sacrifice his interest to the public good, and to the advancement of physics and natural history.

History 1747, pag. 51.

There is scarce a naturalist but hath spoken something of the effect, produced on those that touch it, by the fish called in Latin *torpedo*. The sciences have all their enchanted castles, which the Quixotes in philosophy are eager of assaulting, but seldom can set at liberty the Fair that is included: Such are in physics the tides of the sea, the properties of the loadstone, and such is in natural history the effect of the torpedo.

Of the torpedo or cramp-fish, by Mr. de Reaumur.

Some, and especially the antients, have related it with exaggerations that could gain no reasonable credit; others on the contrary, who have seen and handled this fish in certain circumstances, without feeling any numbness, have spoken of it as of a fabulous fact. There is however no longer any room to doubt of

of it, since such philosophers as Redi and Borelli have attested to the public that they have experienced it. But though the fact hath been established as certain, the cause is yet unknown; which is not an uncommon thing in natural philosophy.

When I was upon the coast of Poictou, in the season when they catch this sort of fish very commonly, I proposed to myself the task of examining it, not with regard to its structure; for we have on that subject a little treatise of Mr. Lorenzini, printed at Florence in 1678, which is almost as complete as one can desire; but what I proposed to myself to examine, was the cause on which depends the numbness that it produces in those who touch it, wherein consists precisely that numbness, and what are the circumstances that accompany it: For the authors that agree in the fact, vary much in its circumstances; some have represented the numbness much greater and others much less; one maintains that the fish doth not operate unless you touch it *immediately*, another pretends that its virtue is even to be feared at a distance. I shall only relate what I have experienced myself, I shall even scrupulously mention all the circumstances; they perhaps will contribute to discover the true cause of this effect.

It may be sufficient in order to give a gross idea of the figure of the torpedo to
those

those who do not know it at all, to say that it is a flat fish which resembles very much a thorn-back. They are of different sizes, but most commonly on the coasts of this kingdom a foot and half long; sometimes there are caught much greater.

I ordered some fishermen to preserve for me alive the torpedos they should catch. I was at a house a league distant from the sea, when they brought me two alive, and in appearance extremely vigorous; yet though I touched them in different places and in different circumstances, I felt not the least numbness. In order to revive their vigour, I put them into vessels full of sea water; they swam there and gave themselves all the motions which fish usually have in water, but still they made me feel nothing extraordinary.

The power however of this fish to occasion numbness appeared to me too well attested even to dare to doubt of it. All that I thought should be concluded from this first experiment, was that my torpedos were weakened, and by being weak had lost their virtue. The most certain method I thought was to examine them in the sea itself. But it seemed as if the torpedos had a mind to make me doubt of their virtue: The first that I touched in the sea, though large and vigorous (for it had never been out of the sea-water) suffered me to handle it at different times, without making me feel any thing

thing extraordinary. I wanted nothing but a little vivacity in forming a judgment, to treat as fabulous all that had been related of it. The torpedo at length fatigued with my repeated touchings, shewed me what she was able to do ; I felt a kind of numbness that suddenly seized my whole arm, from the hand quite to the shoulder, and which even stunned my head : It was very different from a common numbness, it was attended with a considerable, though stupifying, pain. I was unable to move my hand and arm, and in such a situation as the Latins express by the word *attonitus*.

The mention of the word attonitus brings into my mind a beautiful description in Lucan, which I do not remember to have seen observed by any one, though it appears to me extremely striking.

Stantem sublimi Tyrrhenum culmine proræ
Lygdamus excussa Balearis tortor habenæ
Glande petens, solido fregit cava tempora
plumbo.

Sedibus expulsi, postquam cruor omnia
rupit.

Vincula, procumbunt oculi : stat lumine
rpto

Attonitus, mortisque illas putat esse tene-
bras.

The

The thought in the last line, of his imagining the darkness to be the shades of death, is inexpressibly beautiful.

Our feelings can scarce be made known by comparison, yet *this* had some faint resemblance to the painful sensation one perceives when the elbow is struck violently by some hard body. I own ingenuously, that the kind of pain which accompanied it was such, that it lessened my eagerness of making experiments on the torpedo.

The pain however is not of long duration, by degrees it diminishes, and in a few instants intirely vanishes. As soon as my arm was restored, the desire of making new experiments did not fail to revive :

*Luſtantem Icariis fluctibus Africum
Mercator metuens, otium et oppidi
Laudat rura ſui : mox reficit rates
Quaſſas, indocilis pauperiem pati.*

The acquisition of knowledge is the wealth of a philosopher, and it hath not a less influence to make him venture on the hazardous sea of experiment, than the hope of gain which urges the merchant.

The numbness however that it occasioned afterwards, was much less than at the first, and the pain less violent ; perhaps the torpedo was grown weaker.

A learned English anatomist assured the great duke of Tuscany, that the touch of a torpedo had caused in his arm a pain that lasted for two days. Borelli suspects, that the imagination might have augmented the disorder; but might one not suspect likewise, that the increase of the mischief depended on the habit of body in him who made trial of the force of the fish? For Borelli himself tells us, that the anatomist we speak of was attacked with a paralytic trembling.

But how doth the fish operate this numbness? They have hitherto undertaken to give a solution of it in two different manners. The first pretends, that the effect produced by the torpedo depends on an infinite number of corpuscles, which issue continually from this fish, but which are emitted more abundantly under certain circumstances; this is the opinion the most generally received.

I imagine it will please the reader to see this opinion, or more properly that of a torporific virtue, elegantly described by Claudian in a little poem on the torpedo, which is free from the general fault of Claudian, of being too turgid in his expressions.

Quis non indomitam diræ torpedinis artem
Audiit, et merito signatas nomine vires?
Illa quidem mollis, segnique obnixa natatu
Reptat, et attritis vix languida serpit arenis.
Sed latus armavit gelido natura veneno.

Et

Et frigus, quo cuncta rigent animata, medullis

Miscuit, et proprias hiemes per viscera duxit.

Naturam juvat ipsa dolis, et conscia fortis
Utitur ingenio, longeque extenta per algas
Hac confisa jacet: successu læta resurgit,
Et vivos impune ferox depascitur artus.

Si quando vestita cibus incautior æra
Hauferit, et curvis frænari senserit hamis;
Non fugit, aut vano conatur vellere morfu:
Sed propius nigræ jungit se callida setæ,
Et meminit captiva sui, longeque per undas
Pigra venenatis effundit flamina venis.

Per setam vis atra meat, fluctusque relinquit

Absentem tentura virum. Metuendus ab imis

Emicat horror aquis, et pendula fila secutus

Transit arundineos arcano frigore nodos,
Vicitricemque ligat concreto sanguine dextram.

Damnosum piscator onus, prædamque rebellem

Jaçtat, et amissa redit exarmatus avena.

Messieurs Redi, Perrault, and Lorenzini have espoused this opinion: They think that as fire emits abundance of corpuscles proper to heat us, (this is the manner in which they explain their sentiment) so the torpedo throws out

out abundance of little bodies proper to numb the part into which they insinuate themselves; whether it be because they enter in too great a quantity, or because they find passages not well proportioned to their figures.

The second explication is from Borelli; upon the bare recital of it, it will be more to the taste of the mechanical gentlemen. He considers the emission of all these corpuscles as imaginary: And says, that at the time you touch this fish, he is *agitated* himself with so violent a trembling, that he causes in the hand which touches him a painful numbness. *Hæc torpedo digitis compressa tremore adeo vehementi concutitur ut manum contrectantis molesto torpore dolorifico afficiat.* I do not know whether the idea which the words of Borelli have excited in me, is such as he designed to convey; but they have made me imagine, that you perceived a very sensible *agitation* in the torpedo, ready to produce its effect *which* is perhaps some thing like the undulation that there is in chords stretched horizontally, when you draw them out of that position; *tremore adeo vehementi concutitur.* He makes use a little afterwards of the same expression again; *si tangatur piscis eo ipso tempore quo concutitur.* And to mark still better, that there is a visible difference at the time when he is agitated, he says, *si tangatur tempore quo quiescit.*

I have considered attentively the torpedo, in order to determine which of the two opinions I should insist myself of; but in vain have I observed, in vain have I examined this fish, I could never perceive, that he was agitated himself with a trembling, at the time he was prepared to give the numbness: Perhaps the fish that Borelli made his experiments on, being more disturbed than mine, gave himself motions, to which this celebrated author thought he might attribute all the efficacy.

After having well observed the torpedo, I got to know precisely the instant when he was ready to produce the numbness, I foretold it infallibly to those who touched him; and I thought at the same time, that I had divined the whole mystery on which his virtue depends.

The torpedo, like all the other flat fish, is not however absolutely flat; his back, or rather all the upper part of his body is a little convex; I observed, that when he did not, or would not, produce any numbness in those who touched him, his back preserved the convexity which is natural to it: But if he was disposed to act, by imperceptible degrees he diminished the convexity of those parts of his body, which are towards his back, and opposite to his breast; from convex which they were, he made them some-
 times even concave. Then the instant was

Vid. plate
5th,
A.A.A.A

come when the numbness was going to seize the arm ; the blow was ready to be discharged ; the arm dropped dead ; the fingers which pressed the fish quitted their hold ; and instantly all that part of the body of the animal which had been flattened became convex again : But whereas it grew flatter *by degrees*, it became convex again *so suddenly* that one could not perceive the passage from one state to the other ; perhaps the motion of a musket ball, is not more sudden than that of the flesh, which resumed its former situation. When the blow hath been given, and a little before it is given, you not only perceive not the torpedo to be agitated by any violent trembling, as Borelli would have it, but you do not so much as see the least motion on the whole surface of his body. It is the suddenness alone of the blow which produces the numbness.

It is proper to observe on this occasion, that the opinion of Mr. De Reaumur is greatly confirmed by the following fact.

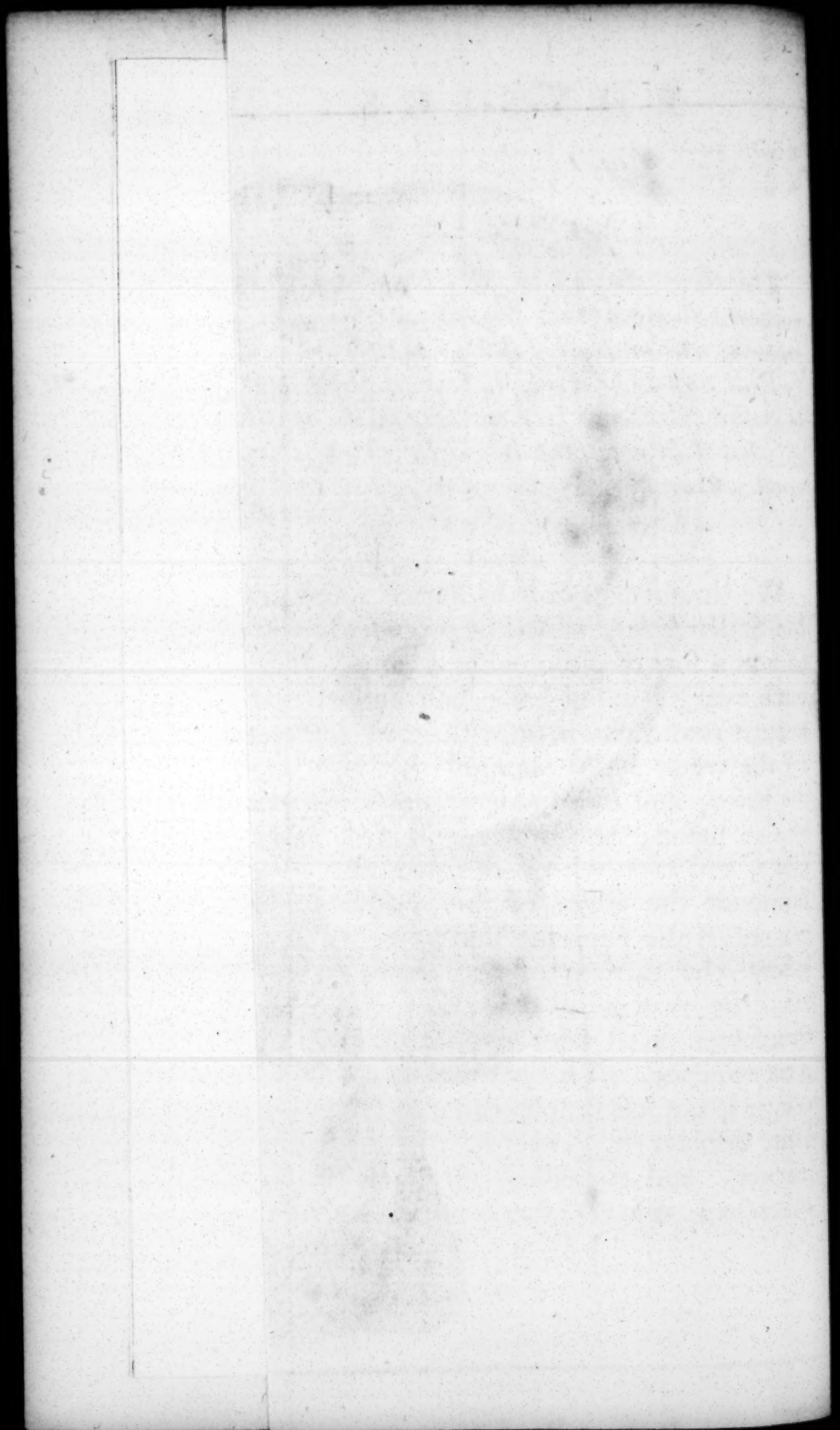
“ In a certain difficult birth, the orifice of
 “ the womb was not sufficiently opened, and
 “ the midwife, who was a very experienced
 “ woman, did all that she could to dilate it,
 “ endeavouring to introduce her hand. The
 “ womb being irritated, contracted itself
 “ sometimes with such violence, that the
 “ midwife drew back her hand quite numb-

Fig. 1.

Pl. V.



J. Mynde sc.



" ed, and to such a degree, that it was a
" whole hour before she recovered its use.

History 1715, pag. 6.

The theory by which Mr. De Reaumur endeavours to explain the phenomenon, namely, by supposing the muscoli falcati, as they are called by Redi and Lorenzini, to be composed of a sort of elastic cylinders, is so precarious an hypothesis, that I do not think it worth the labour of transcribing.

Memoirs 1714, pag. 344.

We cannot pass over in silence a notion of ^{A phys-} Mr. Scheuchzer, which he proposes however ^{cal solu-} only as a sort of philosophic dream. If you ^{tion of the} cause a large round bason half full of water ^{universal} to turn round its center with great swiftness, ^{deluge.} till the water hath acquired the velocity of the bason, and then suddenly stop the motion of the bason; the water will not cease to move, and even with such force, that it will surmount the edges of the vessel. In like manner, if the Supreme Being stopped at any instant the turning of the earth upon its axis, the waters of the sea would diffuse themselves on all sides upon the land with great violence. This way of explaining the deluge is not less simple than new: when the Deity displays his power in an extraordinary manner, and transcends the laws he hath established, it is reasonable to think that the

miracle is executed with the greatest simplicity that is possible.

History 1710, pag. 21.

I have the more willingly extracted the preceding solution, in order to confront it with one by the great Mr. Locke, which I think much more ingenious; and it being in a part of Mr. Locke's writings, where one would not expect to find it, I have met with several philosophers intirely ignorant, that Mr. Locke had ever proposed any.

“ Since the deluge cannot be well explained, without admitting something out of the ordinary course of nature, I propose it to be considered, whether God's altering the center of gravity in the earth for a time (a thing as intelligible as gravity itself, which perhaps a little variation of causes unknown to us would produce) will not more easily account for Noah's flood, than any hypothesis yet made use of to solve it. I hear the great objection to this is, that it would produce but a partial deluge. But the alteration of the center of gravity once allowed, 'tis no hard matter to conceive, that the Divine power might make the center of gravity, placed at a due distance from the center of the earth, move round it in a convenient space of time, whereby the flood would become univer-

“ fal,

“ fal, and, as I think, answer all the phenomena of the deluge as delivered by Moses, at an easier rate than those many hard suppositions that are made use of to explain it.”

Locke's Works, Vol. 3. Pag. 88. Edit. 1727.

Mr. De la Hire read a letter written from Châtillon sur Seine, giving an account of a great storm that had happened there on the 10th of March about 7 o'clock in the evening. An account of two extraordinary tempests.

The head of this storm being all in a flame, the air appeared as red as fire; those who saw it were extremely affrighted, and imagined that the neighbouring villages were intirely consumed by the fire that fell on all sides in little sparks, like those which fly off from red hot iron when beaten on the anvil; after they had fallen they rolled for some time along the ground, and appeared blue; this rain of fire lasted a quarter of an hour, and occupied a large extent of country: At the extremity of the storm it snowed, and the snow fell in large flakes. The same day at Paris there fell about half an hour after 5 in the evening a great quantity of snow accompanied with a sort of hurricane.

On the 17th of the same month, about 4 o'clock in the morning, there fell in several places of the same town of Châtillon a sort

of rain of a reddish liquor, thick, viscid, and stinking, and which resembled a rain of blood. There are seen large drops of this liquor imprinted on the walls, and the same wall was sprinkled with them on both sides; which makes it probable, that this rain was driven by a violent whirlwind.

History Tom. 2. Ann. 1695. pag. 233.

These phenomena give credit to the prodigies related by Livy and other historians, Cœlum visum est ardere plurimo igni. Lib. 3. cap. 5. and, Eo anno cœlum ardere visum; terra ingenti concussa motu est; inter alia prodigia et carne pluit; quem imbrem ingens numerus avium intervalitando rapuisse fertur; quod intercidit, sparsum ita jacuisse per aliquot dies, ut nihil odor mutaret. Lib. 3. cap. 10. Prodigia eo anno multa nunciata sunt—— Romæ in foro boario sanguine pluiffe. Lib. 24. cap. 10.

I must own that I think the cœlum ardere visum of Livy, was nothing else than the aurora borealis or northern lights; for that the ancients were acquainted with the aurora borealis is evident beyond all dispute.

Et varias ignis denso dedit aëre formas,
Nunc jaculum longo, nunc sparso lumine
lampas,
Emicuit cœlo,

*says Lucan lib. 1. ver. 531. and still more
expressly*

expresly Tacitus, Visæ per cælum concurrere acies, rutilantia arma, et subito nubium igne collucere templum. Histor. lib. 5. cap. 13.

As to rains of blood, several historians both ancient and modern have related the like phenomena, and philosophers have endeavoured to explain them by supposing insects in the air carried away by tempests, and dashed against walls and stones.

There are two very curious facts mentioned by Swammerdam, which I believe the reader will be pleased to see, because they come from an undoubted authority.

“ Memini, cum in Galliis olim in saltu
 “ ita dicto Vincennarum versarer, me tum in
 “ equorum aquario circa aquæ superficiem
 “ immensum adeo numerum ejusmodi insecto-
 “ rum (*scilicet pulicum aquaticorum*) ob-
 “ servasse, ut tota aqua verum quasi in san-
 “ guinem mutata videretur: quod equidem
 “ prima fronte videnti terrorem incutiebat;
 “ postmodum vero ansam mihi præbebat ho-
 “ rumce insectorum indolem accuratius in-
 “ vestigandi, mihiq; deinceps circumspecto
 “ cavendi, ne subito nimis de rebus obviis
 “ judicarem: utpote quod innumeris sane
 “ erroribus atque præjudiciis nos intricat.
 “ Quid? si et illi hac ratione decepti forent,
 “ qui pluvias sanguineas nonnunquam ceci-
 “ disse affirmant. Nonne fieri potest, ut
 O 4 “ guttæ

“ guttæ ejusmodi rubræ ab aliis infectis, quip-
 “ pe semper eo tempore, quo recenter è
 “ nymphis prodierunt, guttas sanguineas
 “ stillantibus, profectæ sint? Id vero tum
 “ præprimis obtinere posse videtur, quando
 “ infecta ejusmodi hoc illove anno supra mo-
 “ dum multiplicata sunt: prout circa papili-
 “ ones, muscas, culices, &c. sæpius fieri ex-
 “ perimur.

“ Similem quandam de aqua sanguinea
 “ observationem mecum deinceps communi-
 “ cavit vir clarissimus, Florentius Schuyt,
 “ cum viveret, medicinæ in Academia Lug-
 “ duno-Batava professor; qui mihi retulit,
 “ se tempore quodam studiis intentum rumo-
 “ ris quidpiam percipere, ejusque, quum sen-
 “ sim incretceret, causam sciendi cupidum
 “ reddi; vix tamen hoc sibi venisse desiderii-
 “ um, cum et eodem mox tempore explere-
 “ tur. Ancillarum enim suarum quandam
 “ subito accurrisse, ipsique voce singultibus
 “ interrupta narrasse, quod aquæ Leidenfes in
 “ sanguinem sint mutatæ. Quo quidem in-
 “ tellecto vir memoratus quamprimum sese
 “ in naviculam conferens, loco ipsi indicato
 “ sanguineam illam aquam vitro hauriebat,
 “ eamque sollicitè contemplando nonnisi ru-
 “ bellis animalculis scatere animadvertibat.
 “ Ita subitus terror in diuturnam abiit admi-
 “ rationem.”

Biblia Naturæ, Vol. I. Pag. 89.

In

In the court of the inn at the passage of Accounts of three remarkable fountains. Plougastel between Brest and Landernau there is a well, the water of which rises when the tide of the sea which is very near goes out, and on the contrary, sinks when the tide comes in. This fact is so well established in the country as a prodigy, that Mr. Robelin, a skilful mathematician, hath thought it worthy of examination, and hath sent the account of it to the Academy with an explanation very clear and easy.

The bottom of the well is higher than the *low water* mark in any tide whatever, from thence it happens, that the water of the well, which is a running water, runs out, or in other words, the well sinks when the tide is coming in, which lasts till the tide hath got to the level of the bottom of the well; after that, as long as the tide continues to rise, the well rises with it. When the sea ebbs, there is still a considerable time during which the remains of the sea water, which had advanced pretty far into land and soaked into the ground, fall back successively into the well; which consequently still rises tho' the sea is ebbing. This water hath been so well strained through the ground, that it loses its saltiness; when it is exhausted, the well begins to sink.

History 1717, pag. 9.

The

The marquis of Torcy hath communicated to us a relation, in form of a letter addressed to the Academy, of a curious fountain which is near to Sablé in Anjou, and which is called the *bottomless fountain*; those who have endeavoured to sound it could never find any bottom, and according to the tradition of the country, several cattle that have fallen in could never be found again. It is a sort of gulf, whose mouth is between 20 and 25 feet, situated in the middle and in the lowest part of a heath 8 or 9 leagues in circumference, the borders of which raised in the form of a funnel, descend by an easy declivity quite to this gulf, which is as it were the cistern. The earth trembles commonly all round, beneath the steps of men and animals that walk in this basin. There are from time to time overflowings, which however do not always happen after great rains, and during which there comes forth from the fountain a prodigious quantity of fish, and particularly a great number of pikes speckled, of a very singular kind, and not known in the rest of the country. It is not easy to fish for them, because the ground trembling and sinking at the edge of the gulf, and sometimes at a considerable distance about it, renders the approach extremely dangerous, unless in very dry years. There is reason to believe, that all this ground is as it were the vault of a lake underneath.

History 1741, pag. 37.

The

The marquis de Montalembert hath communicated to the Academy the following observation : In the fountain of *Gabard* in Angoumois, bordering on one of his estates, they often catch pikes that are blind, and never any but what are blind at least in one eye; those that are blind only of one eye, are so all in the right eye; and in those that are intirely blind, you may easily perceive, that the right eye was attacked the first, and is much more damaged than the other. This fountain is a sort of gulf, to which no bottom can be found; several little floating islands of reeds prevent the making use of nets for fishing there, which makes it tedious and difficult to catch the fish, yet the marquis was lucky enough to take a young pike, which was blind in the right eye. It is very remarkable, that this fountain discharges itself by a pretty large out-let into the Liffonne; and notwithstanding this communication, which is very easy, the people of the country declare, that they never take in that river pikes which are blind either in one eye or in both, and that they never take any in the fountain but what are so.

History 1748, pag. 27.

Every thing that regards waters, as well for the necessities of life, as for the ornament of palaces and gardens, hath been always con-

Remarks
on rain-
water, and
on the ori-
gin of

fountains,
together
with some
particular-
ities on
the con-
struction
of cisterns,
by Mr. de
la Hire.

considered as a part of the principal sciences requisite to mankind. Great pains have been taken to render little rivers capable of bearing large vessels, and to join by this means seas very distant one from another. Fountains abounding with water have been conveyed to places where nature had refused them.

*Et, cum exustus ager morientibus aestuat
herbis,*

Ecce supercilio clivosi tramitis undam

*Elicit; illa cadens raucum per levia mur-
mur*

Saxa ciet, scatebrisq; arentia temperat arva,

From hence the stupendous works of aquæducts, the still subsisting monuments of antiquity! Even nature hath been inverted, and machines contrived for raising water and conveying it to the tops of mountains; where sporting in a thousand different shapes, instead of frightening, it now charms us with the representation of the age,

*Omne cum Proteus pecus egit altos
Visere montes;*

and even Æsop hath been taught to moralize in water

But these works were only for the generality of men; the curiosity of those who inquire into the secrets of nature was not yet satisfied;

tified; they wanted to trace the sources of those springs so abounding with water, which are to be found through the whole earth, and even upon lofty rocks. This subject hath given great employment to philosophers both ancient and modern.

There are two *opinions* on the origin of fountains, which are founded *both* upon experiments that seem to be indubitable; for it is evident that several fountains have for their source the fall of rain and the melting of snow upon mountains; but then the question is, how these rains and snows which are very rare upon steep and lofty rocks and in very hot countries, can furnish fountains extremely plentiful and permanent, *or in the bold but beautiful expression of Cicero, fontium gelidas perennitates?*

This is the strongest objection urged by those who are not of the opinion that rains make the fountains, and who espouse this other opinion, that there are subterraneous cavities in the form of an alembic, where the steams of waters, that flow into the earth to a level with the sea, rise up through the chinks of rocks, and are condensed by the cold of the surface of the ground.

Mr. *Mariotte*, who hath followed the former opinion, has made a very particular examination of the water from rain and snow which falls upon that extent of ground which furnishes streams to the river Seine; and he
finds

finds by his calculation, that there is much more water than would be necessary to maintain the river in its middle state during the whole course of a year.

In examining the treatise of Dr. Plot on the origin of fountains, which was printed in the year 1685, I made several remarks which I read at that time to the assemblies of the Academy; and undertook to discover myself *how much* the waters from rain and snow could supply to the fountains and rivers.

I began first, with inquiring what was the quantity of rain that fell on the earth during a whole year, and I have since that time given memoirs of it to the Academy at the end of each year; by which it appears, that the height of water which falls at the Royal Observatory, where I made my observations, would be in a year *at a medium* between 19 and 20 inches: Which is pretty nearly what Mr. Mariotte had *supposed* it to be in his examination.

But as I doubted whether one should reckon upon this quantity of water for the origin of fountains, I made the following experiments to satisfy myself.

I chose a place in the low terras of the Observatory, and, in 1688, I caused to be put into the ground, to the depth of 8 feet, a leaden bason, four feet in surface. This bason had borders 6 inches high, and was a little *inclined* towards one of its angles, where

where I had caused to be soldered a leaden pipe 12 feet long, which having likewise a considerable *inclination* terminated in a little cellar. The bason was placed at a distance from the wall of the cellar, in order that it might be surrounded by a greater quantity of earth a-kin to that which was placed above it, and such as should not be dried by the proximity of the wall. I put into the leaden bason or cistern at the place where the pipe entered several flint-stones of different sizes, in order that *that opening* might not be stopped up when the earth should be laid on above to the height of the ground, *that is to say*, 8 feet high. The soil of this ground is of a middle nature between sand and mere earth, so that the water could easily enough penetrate it, and the external surface is on a level.

I thought that if the waters from rain and melted snow penetrate the ground till they meet with a soft gravel stone or claiety earth, which do not let them pass, as those say who maintain the first opinion on the origin of fountains, the same thing must probably happen to the leaden cistern that I had interred, and that I should have a sort of spring of water, which would flow by the pipe that terminated in the cellar.

But as I was not thoroughly persuaded that this would happen, I put at the same time another machine on trial at the depth
of

of 8 inches only in the ground ; it was a cistern 64 inches in surface. I had chosen a place where neither the sun nor wind had any power, and I had taken great care to remove all the grafs that grew on the ground above this cistern, in order that all the water which should fall on the ground might pass without hinderance to the *bottom* of the cistern, *where* there was a little hole and a pipe, which conveyed into a vessel all the water that could penetrate through the earth. This cistern was not exposed to the air, but was buried in a very large box filled on the sides and underneath with the same earth as was in the inside, in order that the earth of the cistern might not be dried by the air ; to which it would otherwise have been subject on account of the small depth it was placed at.

I remarked in the first trial of this cistern, that from the 12th of June, to the 19th of February following, the water had not flowed by the pipe underneath the cistern, and that on the 19th of February it flowed only on account of a great quantity of snow which was upon the ground and melted. From that time the earth of this cistern was always very moist ; but the water did not flow till some hours after it had rained, and ceased to flow when what had fallen in rain was exhausted.

A year after I repeated the same experiment with the last mentioned cistern ; but I put it 16 inches deep ; which was as deep again as at the first. There was no grass upon the ground, and it was still sheltered from the sun and wind. The same thing nearly now happened as in the preceding experiment, except only that when a considerable time passed without raining, the earth grew a little dry, and a small rain, which might fall afterwards, was not capable to moisten it sufficiently to make the water flow.

Lastly, I planted some herbs in the ground above the cistern ; but when the plants were grown a little strong, not only there did not flow any water after rain ; but all the rain that fell was not sufficient to nourish them : They faded and withered away unless they were watered from time to time.

It then came into my thoughts to measure the dissipation or evaporation of water from the leaves of plants, when they are exposed to the sun and to the wind.

The experiments of the Rev. Mr. Hales on the quantities imbibed and perspired by plants and trees, are so much more ample and entertaining, that I beg leave to refer the reader thither for intire satisfaction on this subject. Vid. Vegetable Statics, from page 4 to 56.

All my experiments convinced me that the water from rains which fall on the ground, where there are always some herbs and trees, cannot penetrate it to the depth of two feet, unless in sandy and stony places, which let the waters pass with ease; but these can be only particular cases, from whence no general consequence can be drawn. An example however of this kind may be seen at the rock of St Baulme in Provence, where the rain that falls on this rock, which is quite split and full of crevices, and where there is no grass, penetrates into the grotto in a very few hours to 67 fathom below the surface of the rock, and there forms a very beautiful cistern; which would make a fountain if the cistern was intirely filled. When there happen on such rocks and in considerable stores great quantities of snow, which melt in the summer by the heat of the sun, remarkable flows of water are observed from some fountains for several hours of the day, and even vicissitudes of flowing if the sun does not shine on these snows but at different hours of the day, the rest of the time the snows lying under the shade of points of rocks and not easily melting. This is, without doubt, the reason of what hath been related, that there are fountains in the midland countries which have a flood and ebb like the sea.

These experiments persuaded me that I was not to expect that the waters from rain
and

and snow would pass through 8 feet of earth, which were above the leaden cistern that I had buried on the terras of the observatory; and indeed there hath not so much as a single drop of water flowed through the pipe for 15 years.

The reader sees therefore that there can be but few fountains which derive their source from rains and snows; and we must necessarily have recourse to other causes, in order to explain how it is possible that there should be *springs* of water extremely plentiful in elevated places, and at a very small depth in the earth; such as is *that* of Rungis near Paris, which cannot be attributed to those grottos or subterraneous alembics, that serve to distil the water of condensed vapours: For there are no rocks in the neighbourhood, as I have discovered by causing several wells to be dug; and there is no need of digging deep, for the water is near the surface of the ground. This spring furnishes a copious and constant stream, and is subject to very little alteration; yet all the extent of territory from whence it could receive its supplies is not sufficiently large to furnish the water of this spring by collecting all that should fall in rain, and even though none of it should be dissipated; but in fact this territory is extremely well cultivated at the same time, and covered with grass and corn. There are some vales very near this place,

where you must dig very deep to find water.

Some have thought to explain these sorts of springs by supposing natural pipes and conduits, which convey the water of some little river placed in an elevated situation, and which passing through places *high* and *low*, and even *underneath* some rivers that cross them, are so well foldered and stopped that they do not let any of this water escape by the way, in order to convey it quite home to the place where it is to have its issue. Now though there might be found such subterraneous places, I am persuaded that they would only have a *declivity* necessary to let the waters flow between the *strata* of earth, till they arrived at some bottom of gravel or clay; but to imagine natural pipes *rising* and *falling*, is only to draw pictures of nature from the model of Versailles, and is attended with this inconvenience, that such pipes, like those of Versailles, would be often subject to be out of order.

I think that another considerable objection might be made to this hypothesis. For if such *high* seated springs, as we have mentioned above, derive their origin from some rivers, these same rivers must likewise receive their waters from other sources still higher; so that the difficulty only skips, like some mountain-nymph, from hill to hill, and eludes the pursuit of the philosopher. For

as to waters from rains and snows melting in places where the bottom may be compact and firm, they can only form some torrents which last but a little while, and can never furnish the continual flow of such streams. The great collections of water, such as ponds, that are commonly at the head of little rivers, prove nothing with regard to the origin of rains; for we have made several experiments which convince us, that there is more water dissipated from a *body* of water exposed to the air in a very large vessel, than there falls into it from the sky.

By Mr. Sedileau's experiments it appears, that in the year 1688, the total quantity of rain that fell was only 11 inches, 16 lines, and $\frac{1}{3}$; whereas the total quantity of the evaporation was 22 inches 5 lines. In the year 1689, the total of rain was 18 inches 1 line, that of the evaporation 32 inches, 10 lines, and $\frac{1}{2}$. In the year 1690, the total of rain was 21 inches, and $\frac{1}{3}$ of a line, the evaporation was 30 inches, 11 lines.

Vid. Memoirs, tom. x. pag. 31.

There remains therefore but one single method of explaining how these abundant sources can be formed in the earth, and even this is attended with some difficulties. We must imagine that there passes through the earth a great quantity of steams or vapours, which rise from the waters that are

there commonly on a level with the nearest rivers or the sea; that these vapours pass the more easily, as they meet with a soil which is more easy to be penetrated; such a rise of vapours may be observed in winter upon opening some very deep caverns. The particles of these vapours may join together, either by the cold of the surface of the earth, when they begin to approach it, or when they meet with a soil that is already full of water to which they join themselves, or lastly if they find some matter that is proper to fix them, as we see that salts exposed to the air retain the particles of water that float in it. By these means the water increasing continually pervades the little veins and openings of the earth, till it finds some *solid ground* to support it; and then escapes on the surface of the earth where that *ground* terminates, or falls back lower into the earth if it can find any vents to discharge itself.

I was willing to see by experiment what one might expect from the vapours of water being condensed in the earth by attaching themselves to stones, that might be full of some salts; for it was a new thought which I had light on for explaining in what manner the waters of vapours that are in the earth might be collected together.

I put into one of the pits in the bottom of the quarry of the observatory a glass vessel,

and I fastened on the brim of the vessel a piece of rag dipped in a little water, wherein I had dissolved some salt of tartar. I chose this salt because I thought it was more proper for fixing the vapours than any other. The place appears to be very moist, and particularly in the summer. Some time after I found at the bottom of the vessel a considerable quantity of a liquor, which was nothing else than the water from the vapour of the air, which had fixed itself to the linnen rag, and having furcharged it, the surplusage, which continually augmented, had flowed down the sides of the vessel. I should have carried the experiment further, to see whether the liquor would have continued to flow, and whether the salt which was in the linnen had been intirely carried away by the water that flowed (tho' I think it possible for stones which may have salts proper for fixing vapours to retain continually their salt, or to load themselves with it a-new) but some one entered into the pit in my absence, broke the vessel, and interrupted my experiment.

I do not enter into the examination of some particular and extraordinary fountains, which are found, as they say, on the sea-shore and on lofty rocks, which have an ebb and flood like the sea, and yet nevertheless are fresh waters; I have formerly explained mechanically how this may happen, by supposing subterraneous reservoirs a little raised above

the level of the sea, and that the cavity where these reservoirs are placed, hath a communication by means of some canals with the sea. For it must happen that when the sea comes *in*, it must compress the air which is in this cavity, which consequently presses upon the water of the reservoir, and obliges it to escape, and even to rise through some chinks or conduits of rocks quite to the surface of the earth, where it forms a fountain, which must reciprocally diminish by degrees in proportion as the sea retires, and as the compressed air, which forced it to rise, recovers its former state.

After reading the preceding theory of Mr. De la Hire on the rise of springs, I imagine it will be agreeable to the reader to see, as it were in the same point of view, the opinion of the great Dr. Halley on the same subject, which I shall transcribe from the philosophical transactions.

“ Those vapours that are raised copiously
 “ in the sea, and by the winds are carried
 “ over the low land to the ridges of the
 “ mountains; are there compelled by the
 “ stream of the air to mount up with it to
 “ the tops of the mountains, where the wa-
 “ ter presently *precipitates*, gleeting down
 “ by the crannies of the stone; and part of
 “ the vapour entring into the caverns of the
 “ hills, the water thereof gathers as in an
 “ alembic

“ *alembic* into the basons of stone it finds,
 “ which being once filled, all the overplus
 “ of water that comes thither runs over by
 “ the lowest place, and breaking out by the
 “ sides of the hills, forms single *springs*;
 “ many of these running down by the val-
 “ leys, or guts between the ridges of the
 “ hills, and coming to unite, form little *ri-*
 “ *vulets* or *brooks*; many of these again meet-
 “ ing in one common valley, and gaining
 “ the plain ground, being grown less rapid,
 “ become a *river*; and many of these being
 “ united into one common channel, make
 “ such *streams* as the *Rhine*, the *Rhone*, the
 “ *Danube*; which latter one would hardly
 “ think the collection of waters condensed
 “ out of *vapour*, unless we consider how vast
 “ a tract of ground that *river* drains, and
 “ that it is the sum of all those *springs*,
 “ which break out on the south side of the
 “ *Carpathian* mountains, and on the north
 “ side of the immense ridge of the *Alps*,
 “ which is one continued chain of mountains
 “ from *Switzerland* to the *black-Sea*. And
 “ it may almost pass for a rule, that the
 “ *magnitude* of a *river*, or the quantity of
 “ water it evacuates, is proportionable to the
 “ length and height of the ridges from
 “ whence its fountains arise. Now this *the-*
 “ *ory* of *springs* is not a bare *hypothesis*, but
 “ founded on experience, which it was my
 “ luck to gain in my abode at *St. Helena*;
 “ where

“ where in the night-time, on the tops of
 “ the hills, about 800 yards above the sea,
 “ there was so strange a condensation, or ra-
 “ ther precipitation of the *vapours*, that it
 “ was a great impediment to my *celestial ob-*
 “ *servations*; for in the clear sky, the *dew*
 “ would fall so fast as to cover, each half
 “ quarter of an hour, my glasses with little
 “ drops, so that I was necessitated to wipe
 “ them off so often, and my paper, on which
 “ I wrote my observations, would immedi-
 “ ately be so wet with the *dew* that it would
 “ not bear ink; by which it may be suppo-
 “ sed how fast the water gathers in those
 “ mighty high ridges I but now named.—
 “ I doubt not but this *hypothesis* is more rea-
 “ sonable, than that of those who derive all
 “ *springs* from the *rain-waters*, which yet
 “ are perpetual and without diminution, even
 “ when no rain falls for a long space of time:
 “ or than that which derives them from a fil-
 “ tration, or *percolation* of the *sea waters*
 “ through certain imaginary tubes or passages
 “ within the earth, wherein they lose their
 “ saltiness; this besides many others labour-
 “ ing under this principal absurdity, that the
 “ greatest *rivers* have their most copious
 “ fountains farthest from the sea, and whi-
 “ ther so great quantities of fresh water can-
 “ not reasonably be derived any other way
 “ than in *vapour*. This, if we may allow
 “ *final causes*, seems to be the design of the
 “ *hills*,

“ *bills*, that their ridges being placed through
 “ the midst of the *continents*, might serve as
 “ it were for *alembics*, to distil *fresh water*
 “ for the use of man and beast, and their
 “ heights to give a descent to those *streams*
 “ to run gently, like so many veins of the
 “ *macrocosm*, to be the more beneficial to the
 creation.

Vid. Lowthorp's Abridgement, Vol. 2.
 pag. 127.

I have been sufficiently ample on the ori-
 gin of *springs*, and shall now unfold some
 particular remarks I made on the advantages
 that may be derived from rain-water. The
 most considerable advantage of rain-water is
 the facility of collecting it into subterraneous
 reservoirs, that are called cisterns, where af-
 ter it hath been purified by passing through
 fine sand, it preserves itself several years
 without corrupting. This water is common-
 ly the very best that can be used either for
 drinking or for employing in several uses,
 such as washing and dying, for this reason
 because it is not mixed with any salt from
 the earth, as *are* almost all the waters from
 springs, even such as are esteemed the best.
 These cisterns are of very great use in places
 where there are no *springs*, or where the
 waters of the wells are bad. This is not the
 place for speaking of the construction of ci-
 sterns, nor of the choice of materials which
 should

Some use-
 ful obser-
 vations on
 cisterns.

should be employed in them; all that is requisite in our inquiry is, to have a place which will hold water, and that the stones and mortar with which they are cemented may not communicate any bad quality to the water that lodges there for a considerable time.

Those who have cisterns, and are curious of having good water, observe carefully to let no water enter into their cisterns from melted snow or stormy rains. With regard to the water from melted snow, I think they are in the right to exclude it from the cistern, tho' not on account of the salts which are imagined to be included and mixed with the particles of the snow; but only because these snows continue several days, and sometimes whole months, upon the tops of houses, where they grow corrupt by the dung of birds and other animals, and still much more by the long abode they make upon the tiles which are always very dirty. For this reason when it begins to *rain*, I would advise that the first water which comes from the roof and must enter into the cistern, should be thrown away as bad, having only served to wash the roofs which are covered with the dust, that rises from mud dried in the streets and public roads, and that what comes afterwards should be the only water reserved in the cistern.

There is another very considerable observation with regard to water that ought to be rejected from cisterns, which chance alone hath

hath discovered to me. I have for several years been curious to collect the rain-water that fell at the observatory, by means of a cistern which I make use of for measuring the quantity of water that falls in a year. This cistern is made of iron plates, * well tinned, it hath 4 feet in surface, and borders 6 inches high. It hath an hole and a little pipe which is soldered to the hole towards one of its angles, by which the water that falls into the cistern, which is a little inclined towards that angle, is conveyed into a vessel which receives it, in order to be measured afterwards, and to discover by this means the quantity that hath fallen. I cleansed and washed the cistern, and the vessel which receives the water, as expeditiously as possible, at the beginning of a rain which had the appearance of abundance ; and I afterwards collected the water into glass bottles that were clean, in order to preserve it. But upon my being willing to taste some of this water, I was surpris'd at its having a bad taste, and that it smelt of smoke ; which appeared to me very extraordinary, for I had often tasted of what had been collected in the same manner, and which had not the same taste. I could think of nothing that could communicate this odour of smoke to the rain-water ; for the place where I collect it is very open and very elevated, and there is no chimney

* In French *Fer blanc*.

but

but at a very considerable distance. At last I considered, that this rain-water had fallen with a north wind, which is not very common, for it rarely rains with that wind; and as all the city of Paris is situated to the north of the observatory, that the smoke of chimnies had mixed itself with the rain that fell; and that this was the true cause of the disagreeable odour of the water: For we know by several experiments, that water very easily takes the smell of smoak. In fact, I assured myself of it some time after; for having collected again some rain-water that fell with a south or south-west wind, I found in it no such taste; because towards the south of the observatory there are only extensive fields, *or in the beautiful language of Cicero, immensitate camporum.*

I conclude from hence, that there ought to be rejected from cisterns, all those rain-waters which were brought by winds that pass over places infected with any bad smells, such as from common-sewers, lay-stalls, and even great cities on account of the smoke, for the exhalations and noxious vapours which mix themselves with the water that enters into the cistern, must corrupt that which had entered at a better season.

Memoirs 1703, pag. 56.

Observation of the figure of

It hath been long known, that snow is hexagonal; but it hath not perhaps yet been observed

observed, that the six *radii* of which each ^{snow, by} flake is composed, are often as it were so ^{Mr. Caffi-} many little branches garnished with leaves, ^{fini.} and that some flakes form a kind of flower: this Mr. Caffini hath remarked by observing with a microscope the snow which fell on the 1st of February 1692. The two figures that we have annexed, will make the reader ^{Vid. plate} comprehend at one view what a long dis- ^{6th.} course perhaps would scarce be able to explain.

Memoirs Tom. 10, pag. 37.

Mr. Sedileau hath remarked from experi- ^{Of the} ments which he hath made, that six inches ^{quantity} of snow in height commonly yield no more ^{of water} than about an inch of water, when the snow ^{from} is melted: This must be understood of snow ^{snow.} such as it falls naturally, without being trod or pressed by any thing but its own weight. It is true, that the proportion we have mentioned depends on the manner in which the snow falls; for when it falls in large flakes, it heaps up more; and by consequence yields more than when the flakes are finer.

Ibidem, pag. 34.

According to the observations of Mr. De la Hire likewise, snow when melted is reduced to the fifth or sixth part of the height it had. Yet in the night between the 13th and 14th of February in the year 1711 there fell snow, which was reduced to about the 12th part of its

its height, *that is to say*, by melting it diminished as much again as it usually doth. The reason is, as Mr. De la Hire hath remarked, that it was very fine, very small, and all in little threads (*filets*) extremely dry, which supporting one another took up a great deal of space.

History 1711, pag. 16.

Of the
meteo-
logical ob-
servations.

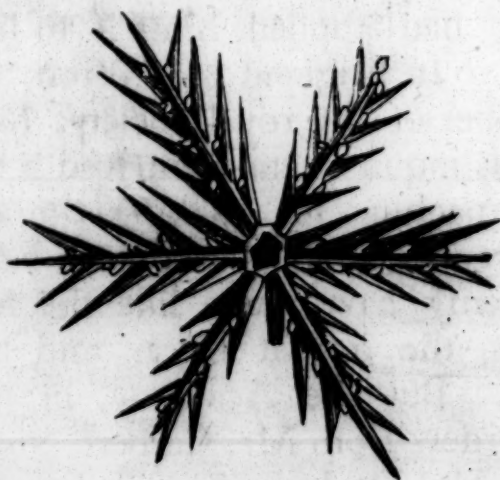
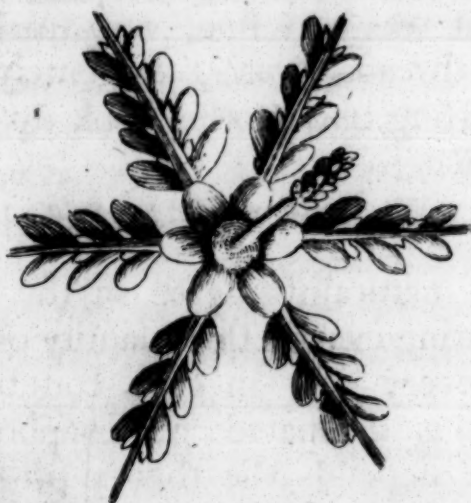
All naturalists agree in the usefulness of measuring exactly the quantity of water which falls every year in each country, and how much is dissipated by evaporation; from thence depends the theory of fountains, rivers, the sea, vapours, the nourishment of plants, droughts, &c.

Father Cabée a jesuit and a skilful philosopher, had applied himself to such observations. In England Mr. Wren, an illustrious member of the royal Society, hath laboured in this inquiry, and contrived a very ingenious machine for making the experiments. In France, Mess. Mariotte and Perrault of the Academy have made likewise some experiments, the one at Dijon, and the other at Paris. But the Academy, in execution of the orders from Mr. Colbert, and afterwards from Mr. De Louvois, who were sensible of the usefulness of such an inquiry, renewed with great assiduity these experiments, which she hath continued ever since.

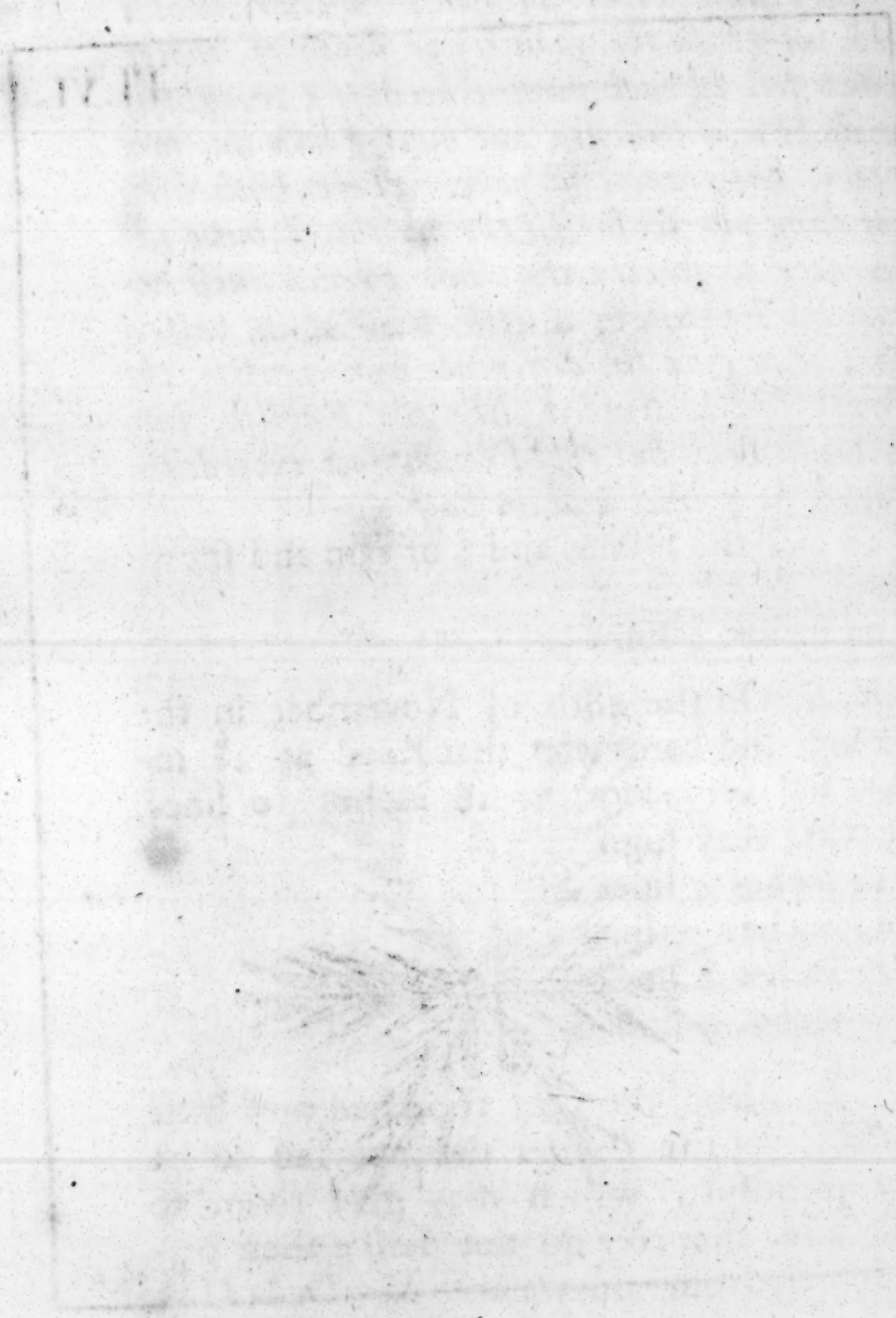
History Tom. 2. pag. 133.

The

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The reader will be pleased to observe, that I have not given the quantity or depth of water which fell in each month, because I thought it would be only tedious; but have given the sum total of the year, and where there hath been any thing particular in the months, I have taken care to specify it. The French inch exceeds the English by a little more than half a line; or to give the difference more exactly, the French foot is divided, like the English, into twelve inches; but the French foot exceeds the English by 7 lines and an half.

A register of the quantity of rain that hath fallen each year, and several other meteorological observations for 60 years, by Mr. De la Hire.

18 inches 11 lines and $\frac{1}{2}$ of rain and snow-water. Anno 1689.

23 inches 3 lines $\frac{3}{4}$. 1690.

N. B. On the 28th of November in the evening, the barometer that stood at 28 inches, fell very soon to 26 inches 10 lines, the wind very high.

14 inches 5 lines $\frac{1}{4}$. 1691.

22 inches 7 lines $\frac{1}{2}$. 1692.

22 inches 8 lines 1693.

11 inches 9 lines $\frac{1}{2}$. 1694.

N. B. Altho' the year 1694 had very little rain fall, yet the *springs* did not fail to be very plentiful; which may give room to conjecture, that *they* do not derive their origin intirely from rain-water. Mr. De la Hire observed a singular variation in his barometer; for in the beginning of November, in 24 hours time, the mercury fell from 28 inches

2 lines to 26 inches 10 lines, which makes 16 lines difference. In a few days after it ascended to its former height of 28 inches 2 lines. There were at that time very high winds.

Anno
1695.

19 inches, 7 lines, $\frac{3}{4}$. The thermometer on the 6th of February at 7 degrees.

N. B. The three first months of this year have given, contrary to what is usual, much more water than the months of June, July, and August. Those first months likewise have been abundant with snow. On the 30th of July at night there fell 14 lines of water. During the summer, the west and north-west winds have so diminished the heat, which is never considerable but with a south-east wind, that the fruits had not their perfect maturity.

1696.

19 inches 5 lines $\frac{1}{2}$. The thermometer was at the lowest on the 28th of December at 23 degrees and $\frac{1}{2}$; at the highest on the 30th of August at 61 degrees. Note, That the thermometer is at the freezing point at 32 degrees.

N. B. The month of July has been very dry, contrary to what is usual, there having fallen only in this month 8 lines and $\frac{1}{4}$. The cold of the winter very moderate in comparison of preceding winters.

1697.

20 inches 3 lines.

In

In the month of June the rain was very abundant. The greatest heat in the whole year was on the 14th of May.

21 inches 9 lines.

Anno
1698.

18 inches 8 lines $\frac{1}{4}$. The thermometer on 1699. the 11th of December at 25 degrees and $\frac{1}{2}$; on the 25th of July at 63 $\frac{1}{2}$; Note, That the observations of the thermometer are made a little before sun-rising. In the barometer, which is placed nearly 120 feet above the level of the river, the greatest height of the mercury was 28 inches 3 lines on the 21st of November and the 31st of December; and its greatest fall to 26 inches 9 lines on the 14th of January and the 14th of December: Consequently the difference of the *heights* of the mercury between the *greatest* and the *least* was 18 lines. On the 23d of October the declination of the compass was 8° 10'. The needle made use of is 8 inches long, and one of the most excellent that hath been made.

Although the quantity of water which fell this year, was but very little less than what falls at a *medium*; yet it appeared to be a very dry year, the reason is that the rains were not *continued*, the greatest having fallen in one day; and the intervals were very considerable.

In the year 1699, there was a very remarkable endemic scurvy at Paris; of which

a particular account will be given in the medicinal part of this work.

1700.

20 inches.

There fell but one line and an half during the month of September ; from whence the drought was very great towards the end of the summer. The cold was not considerable during the whole winter. The greatest heat was on the 21st of July, the thermometer marking 61 degrees and $\frac{1}{4}$. As to the barometer, its greatest height was on the first of January at 28 inches 4 lines and more than $\frac{2}{3}$, with a gentle wind at east-north-east, and it kept very high during this whole month, tho' the wind often shifted to the west, but always inclining a little to the north ; yet it was not cold at the same time, for the air was all the while in a temperate state, and sometimes even warm. The barometer at the lowest was 26 inches 8 lines $\frac{1}{2}$ on the 26th of November, with a high wind from the south and a little snow which melted immediately. Thus the difference between the greatest rise and the greatest fall of the mercury in this year was one inch and somewhat more than 8 lines.

The declination of the needle was 8 degrees 12 minutes towards the west on the 20th of November.

1701.

21 inches 4 lines $\frac{1}{4}$.

This

This year appeared extraordinary on account of its drought in the spring; yet in general it hath been one of the most rainy years we have had a long time. The most considerable thing that happened this year was the hurricane on the 2d of February; the wind was very violent, and the barometer at 27 inches 4 lines $\frac{2}{3}$; which is extraordinary, because in high winds the barometer generally sinks very low.

As to the cold it was not considerable, for it scarcely froze. My thermometer marks the beginning of freezing at 32 degrees of height, and it did not descend lower than to 28 degrees and $\frac{1}{2}$; whereas in a great frost, such as sometimes happens in this country, it descends even to 7 degrees, as was the case on the 7th of February 1695.

Towards the end of January, and in the beginning of February, which is the season of the greatest cold, my thermometer was often at 40 degrees. It is remarkable likewise, than on the last day of November the warmth was as great as on the 12th of June.

The heat in the months of July and August was extraordinary; for this same thermometer was very often mounted up to 65 degrees; and on the 1st of September it was at 65 degrees $\frac{1}{2}$. This thermometer is always exposed to the air, but in a place very well sheltered, where neither wind nor sun have any power, and all the observations are

made a little before sun-rising. The barometer was in its lowest state this year, at 26 inches 10 lines, on the 6th of March, and in its highest at 28 inches 2 lines $\frac{1}{2}$ on the 9th of February. The difference therefore between the highest and the lowest was no more than one inch 4 lines and an half.

The declination of the needle was 8 degrees 48 minutes towards the west on the 22d of September.

Anno
1702.

16 inches $\frac{1}{2}$.

This year hath been considered as one of the most extraordinary ever known for the dryness of the spring and summer ; yet it hath been one of the most fertile in grain, except such as is sown in the month of March ; which may be attributed to the coolness, and natural moisture of the greatest part of the lands of this country. But moreover it is to be observed, that very often the three months of June, July, and August, furnish as much rain as all the rest of the year, which did not happen this year : Now the rains of those three months generally falling in hasty showers contribute very little to the nourishment of plants, but run off in torrents, or quickly exhale by the heat of the ground and air.

The thermometer at the lowest this year was at 14 degrees and $\frac{1}{2}$, on the first of January, which was the mark of very great cold ; but on the 6th it was got up to 43 degrees, and kept itself all the rest of that month, and

and even the beginning of February at about 40 degrees, sometimes a little more, sometimes a little less. On the 25th of December following it was at 44 degrees $\frac{1}{4}$, which was within 2 degrees of the height it was at in the first days of June. On the 19th of June it was at 60 degrees $\frac{1}{2}$, on the 29th of July at 61, and the 6th of August at 62, which was the highest it rose; but on the 2d of September it was still at 61 $\frac{2}{3}$. From whence it may be concluded, in general, that during the months of June, July, August, and the beginning of September, the heats were very great. The greatest elevation of the liquor of the thermometer was at about two of the clock in the afternoon to 71 degrees $\frac{1}{2}$, on the 5th of August. The new mercurial thermometer of Mr. Amontons was at the same time at 31 inches 5 lines. The mercury of my barometer was then at 27 inches 10 lines $\frac{1}{4}$.

In the barometer (*which is now mentioned to be placed near 156 feet above the level of the river in its mean state*) the mercury rose in the tube to 28 inches 2 lines $\frac{2}{3}$, on the 11th of February. The wind was then to the south, as it was some days before and after, tho' the barometer still kept itself at about 28 inches; which doth not correspond with what is usually observed of the height of the mercury in the barometer with such a wind. It was when lowest at 26 inches 5 lines on the 20th of December, with a moderate south wind,

wind, which is very extraordinary; for the great sinking of the mercury seemed to mark some violent storm: The difference therefore between the highest and the lowest was 1 inch 9 lines $\frac{1}{2}$, which is much more than usual.

The declination of the needle was on the 22d of September, 8 degrees 48 minutes, towards the west.

Anno
1703.

17 inches 4 lines $\frac{1}{4}$.

The cold was not considerable during the whole winter, for my thermometer did not descend quite to 26 degrees. The heat likewise was moderate, and but of short continuance: The hottest day was the 12th of August, when the thermometer rose to 64 degrees. I repeat again that my observations of the thermometer are made at about sun-rising, which is the time when the air is the coolest; and between 2 and 3 of the clock in the afternoon is the greatest heat of the day. During the summer I have remarked that between 2 and 3 of the clock in the afternoon the thermometer hath rose 10 or 12 degrees more than it was in the morning at sun-rising, tho' the thermometer is always kept in the shade.

The barometer was at the highest on the 10th of December, in the evening, at 28 inches 4 lines $\frac{1}{2}$: On the 3d of January it was at the lowest in the whole year at 26 inches 6 lines $\frac{1}{2}$, with a little rain, but no tempest, as there often is when the mercury is very low. Thus the difference between the greatest, and the

the least height of the barometer this year was 1 inch 9 lines $\frac{1}{2}$, which is a little more than usual, for it seldom exceeds 1 inch and 6 lines.

I observed on the 18th of December the declination of the needle to be 9 degrees 6 minutes towards the west.

In this year, 1703, between the 26th and 27th of November, was the famous high wind here in England; the Academy takes no notice of any storm at that time, and therefore I presume it was not felt at Paris. Indeed Mr. Addison in his noble simile of that tempest seems particularly to confine it to Britain:

So when an angel by Divine command
With rising tempests shakes a guilty land,
Such as of late o'er pale Britannia past,
Calm and serene he drives the furious blast;
And, pleas'd th' Almighty's orders to perform,
Rides in the whirl-wind, and directs the storm.

19 inches 10 lines.

During the year 1704, the rain that fell Anno
1704. was pretty equally distributed through the several months, except the two of July and October, when it rained very little. The dryness of the last is very useful for sowing; *which by the way is the meaning of that expression in Virgil,*

Nudus ara, fere nudus.

And

And the blockhead who turned it into ridicule with Habebis frigore febres, deserved to be sent to plough in the wet for his pains.

The prevailing wind through the whole year was from the west, as it commonly is in these countries, and it is with such sort of winds that it commonly rains. But the rains which have been the most abundant *at once*, but which did not exceed an inch in height, came with a northerly wind.

The barometer often rose above 28 inches, but it rose the highest on the 25th of December, in the morning, to 28 inches, 3 lines, $\frac{1}{6}$; and it was the lowest on the 25th of November, at 26 inches 11 lines: So that all the difference between the highest and the lowest was only 1 inch, 4 lines, $\frac{1}{6}$.

The most considerable thing with regard to the barometer, which marks the weight of the air, is the *changes* that happen to it in two or three days, wherein we often see it descend, and rise more than an inch. This shews that there must be great variations in a little time in the height of the atmosphere: For in order to solve these different weights of the air, it doth not appear to me probable to suppose, as some philosophers do, different liquids, and of different gravities, on the surface of the earth, which are sometimes carried one way, and sometimes another; for we know by observation that the
air

air is commonly lightest when most loaded with vapours.

I think one may very well explain, in the following manner, all that we observe of the weight of the air, or atmosphere, in all its circumstances. We know by very exact observations that the barometer rises in general less high between the tropics * than in the northern countries; from whence it may be conjectured, that the figure of the atmosphere is a long spheroid, whose axis corresponds with that of the earth. Now as wherever there is air, there may be winds; if the same wind reigns through the whole mass of air, and comes from the south, it will necessarily *lower* the height of the atmosphere in *these* countries: And on the contrary, if it comes from the north, it will *raise* it. But moreover as winds from the south bring *us* rain, it will follow that it should rain when the air will appear lighter: The intire contrary must happen from the opposite quarter.

This is in general what must follow from this supposition; but if the south wind reigns only near the surface of the earth, and there be a northerly wind in the superior region, it may rain altho' the air appear very heavy; and by a contrary reason it may be very fine weather

* I do not know from whence Mr. De la Hire got his observations; but it appeared from the observations of Mr. Bouguer, and Mr. De la Condamine, in the 1st volume, that the mercury generally stood at the equator on a level with the sea at about 28 inches.

weather with a northerly wind, and the barometer extremely low, for we can only observe the winds that are near the surface of the earth.

That there are different winds that rule in the air at the same time, and sometimes entirely opposite ones, is frequently evident from the driving of the clouds in different directions. It may happen that the combat of these contrary winds, when they are extremely violent, may occasion storms and hurricanes; *which Virgil seems to allude to in his description of the storm,*

“ Una eurusque notusque ruunt, creber-
 “ que procellis
 “ Africus.

And Horace expressly says,

—Nec timuit præcipitem africanum
 Decertantem aquilonibus.

Virgil hath elegantly shewn his skill in philosophy, and at the same time in physic, with regard to the influence of the air on animal bodies, in the following lines.

“ Verum, ubi tempestas et cæli mobilis
 “ humor
 “ Mutavere vias, et Jupiter uvidus austris
 “ Densat, erant quæ rara modo, et, quæ
 “ densa, relaxat;
 “ Vertuntur species animorum, et pectora
 “ motus

“ Nunc

“ Nunc alios, alios, dum nubila ventus
 “ agebat,
 “ Conciunt. Hinc ille avium concentus
 “ in agris,
 “ Et lætæ pecudes, et ovantes gutture corvi.

The thermometer was at the lowest on the 23d of January at 14 degrees $\frac{2}{3}$; but it quickly rose again to about 30 degrees, the freezing point being at 32. The heat was the greatest on the 28th of July, the thermometer having rose before sun rising to 66 degrees $\frac{1}{4}$.

The declination of the needle was on the 30th of October 9 degrees 20 minutes to the west. The quantity of rain that fell this year at Pont-briant near St. Malo, was 23 inches, 8 lines, $\frac{1}{4}$; at Lyons 15 inches, 4 lines, $\frac{1}{4}$.

13 inches, 10 lines, $\frac{3}{4}$.

Anno
 1705.

Yet the harvest hath been very plentiful, notwithstanding the dryness of the year; which may be attributed to the great rains in the month of April.

Mr. De Plantade hath written to Mr. Casfini an account of the excessive heat this summer at Montpellier, particularly on the 30th of July; there had never been any thing like it in the memory of man. The air was that day almost as burning as what issues from the furnace of a glass-house. A thermometer of Mr. Amontons, which Mr. De Plantade had, tho' it was in a place where the
 4 air

air did not enter freely, rose very near to the degree where tallow is marked to melt. The greatest part of the vines were scorched on that day, which had never happened in that country. The astronomers of Montpellier remarked, that during this so burning a summer the pendulums gained considerably.

At Paris the 6th of August was much hotter than the 30th of July. A thermometer of M. Hubin, which Mr. Cassini had made use of for 36 years, broke about two o'clock; and the greatest part of the thermometers with spirits of wine were broke; Mr. De la Hire's was at 80 degrees about 3 in the afternoon, tho' it was in the shade, and exposed to the air in the open tower of the observatory. The greatest height of the thermometer in the morning about sun-rising, was on the 18th of August at 65 degrees $\frac{1}{2}$; but rose to 75 about three in the afternoon, as it constantly did during the latter end of July, and the beginning of August.

It must be observed, that the greatest *heat* of the afternoon is not always corresponding with *that* of morning, which is owing to several particular causes.

The lowest state of the thermometer was on the 2d of February, at 25 degrees, and only at 30 degrees the 13th of November following, when it froze pretty hard; but it soon got up to 40 degrees.

The mercury in the barometer was at its highest the 28th of February, at 28 inches, 3 lines, $\frac{1}{3}$, with a low wind, N. N. E. and at its lowest the 20th of December, at 26 inches, 7 lines, $\frac{5}{8}$, with a wind W. S. W. Thus the difference between the highest and the lowest was 1 inch, 7 lines, $\frac{1}{2}$, pretty nearly what is usual.

I shall remark here by the way, that there are barometers in which the mercury rises a good deal higher than in others, at the same time, and in the same place, tho' they are made with the greatest exactness. (*Vid. Vol. 1st, Page, 27.*)

After all the experiments that we have made on barometers with different mercuries in the same tube, and with different tubes and the same mercury, we think it probable that the different heights of mercury in the barometer tubes arise only from the nature of the glass, the pores of which are not equally close; and that the air is not only composed of two different matters, the one intirely *gross* and the other intirely *subtil*; but that the particles of the gross matter having different degrees of *grossness*, 'till it becomes subtil, the pores of some glasses let pass a part of the grosser matter, which by its weight depresses a little the mercury in the tube. This hypothesis seems confirmed by one of the last experiments that the deceased Mr. Amontons made at the academy,

with

with the *barrel* of a musket well soldered at one of its ends instead of a glass tube, *where* it was remarked, that the mercury stopped much lower than in the common glass tube; perhaps because the pores of iron are much larger than those of glass.

I observed the declination of the needle to be 9 degrees 35 minutes towards the west on the last day of December.

The total quantity of rain that fell this year at Pont-briant near St. Malo was 260 lines.

At Lyons 22 inches 8 lines $\frac{1}{2}$.

Anno
1706.

15 inches 3 lines $\frac{5}{8}$.

This year may be considered in general as a dry one; but as little rain fell in July and August in comparison to what is usual in those months by hasty storms, it may be considered as a very wet year.

The thermometer was on the 21st of January at 20 degrees $\frac{1}{2}$, but it almost immediately rose to about 30 degrees.

Although the cold was not considerable and but of little duration, yet the heat was very great and lasted a long time, the thermometer having almost always kept at about 60 degrees through the three months of June, July, and August. The hottest day was the 8th of August, when the thermometer was at 68 degrees about sun-rising, and the same day at 2 of the clock in the afternoon it was got up to near 82 degrees.

The

The barometer was on the 10th of March at 28 inches 1 line $\frac{1}{2}$, and on the 22d of December it was sunk to 26 inches 9 lines; the difference between the two heights was therefore 1 inch 4 lines $\frac{1}{2}$; which is pretty nearly what is common, but it seldom descends so low, unless there be an high wind, and which continues a long time, towards the south, as it then was. I have very often remarked, that the mercury hath been very high, tho' the wind hath been southerly; which is contrary to the usual rule.

On the 31st of December the declination of the needle was 9 degrees 48 minutes towards the west.

17 inches 11 lines.

Anno

Scarce any rain fell in the month of April, 1707, and but little in May; yet the year hath been very fertile in corn; as it commonly happens in these countries, because the greatest part of the lands are cool and moist. The cold weather was very moderate, the thermometer having never fallen below 27 degrees $\frac{1}{3}$; but the heat in summer was excessive, especially on the 21st of August, when the thermometer was got up to 69 $\frac{1}{3}$ at sun-rising, and about 3 in the afternoon to 82.

The barometer was at 28 inches 3 lines $\frac{1}{3}$ on the 21st of November, which is pretty near the greatest height that it rises *here*; the wind was then towards the west and the sky serene, but the preceding and following days

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the

the wind inclined to the north. The *lowest* that the barometer fell was only to 27 inches 1 line on the 4th of December, which is much less than it sometimes falls; the wind was *then* to the south-west, and a very little rain.

The declination of the needle was 10 degrees 10 minutes towards the west on the 28th of December.

Anno
1708.

18 inches $\frac{1}{4}$.

At Pont-briand near St. Malo, there fell 24 inches 6 lines. At Lyons 36 inches 9 lines. By which it appears that there hath fallen double the quantity of rain at Lyons, to what hath fallen at Paris; which may reasonably be attributed to the great *mountains* that are but at a little distance from Lyons, *where* there always falls much more rain and snow than in flat countries.

The thermometer was at its *lowest* in the beginning of the year on the 13th of February at 27 degrees $\frac{1}{2}$, but before that day and after it between 35 and 40 degrees; on the 12th of December following, the thermometer descended to 25 degrees, which was the greatest frost of the whole year, and therefore the cold of the year was not very considerable. The greatest heat was on the 15th and 16th of August; the spirit of wine in the thermometer rose to 66 degrees $\frac{1}{2}$ about sun-rising, and about 3 o'clock in the afternoon it rose to 76 degrees. Thus the heat and cold of
this

this year have been pretty much alike in a moderate state.

My barometer was at its *lowest* on the 10th of January at 26 inches 9 lines $\frac{1}{2}$, with a gentle south-east wind; and at its *highest* on the 17th of November at 28 inches 1 line $\frac{5}{8}$, with a low north N. E. wind, but the days preceding and following a southerly wind: So that the difference between the highest and lowest was 1 inch 4 lines $\frac{1}{2}$ *nearly*.

The declination of the needle on the 27th of December was 10 degrees 15 minutes to the west.

21 inches 9 lines $\frac{5}{8}$.

Anno

In the three months of April, May, and June, there fell almost as much rain as in all the other nine (which is a circumstance that commonly holds true with regard to the months of June, July, and August); and was the reason of the March seeds, which were sown very late this year, producing a very plentiful crop. The great quantity of snow, which fell in the winter, perhaps contributed to this fertility of the earth; and if the wheat and rye had not been frozen quite to the root, this year would have been extremely plentiful.

1709.

The cold in the beginning of the year was excessive; for my thermometer was sunk to 5 degrees on the 13th and 14th of January, and the following days having risen a little, it returned back to 6 degrees on the 20th, and on the 21st to 5 degrees $\frac{3}{4}$, but afterwards

the cold diminished by very flow degrees. On the 4th of January the thermometer was at 42 degrees, on the 6th it came to 30, on the 7th to 22, the 10th to 9, and at last the 13th to 5 : The reader will please to remember, that the freezing point is at 32.

It is extremely surprising, that this cold came on without any considerable wind, or only a very slack wind from the south, and when the wind increased and turned to the north the cold diminished.

To solve it, Mr. De la Hire said that the mountains of Auvergne, which are to the south of Paris, were at that time all covered with snow ; and Mr. Homberg observed that a very cold northerly wind blowing over a vast continent having preceded, the south wind was only a reflux of the same air as the north wind had driven, and which had not been warmed in any country. These two causes may very well be supposed to co-operate.

Another surprising thing was that notwithstanding the extreme violence of the cold, the river Seine was not intirely frozen at Paris, but the middle of its current continued free, except that there floated in it large pieces of ice ; yet in less rigorous winters, it hath been often so frozen, that carriages could pass over it. Mr. Homberg thinks that large rivers, whose currents are very strong, would never freeze in their chanel, at least in our climate,

climate, were it not for the flakes of ice that are broken off from the edges of the river, and carried into the chanel, besides the large pieces of ice which are brought down from lesser streams that fall into the greater; all which being stopped either by a bridge, or an elbow of the river, or by any other obstacle, catch and fasten one to another, and form a crust that covers the whole surface of the river. Now the cold of this winter was so sudden, and the ice at the edges, and in the lesser rivers, was so fast bound at once, that nothing of that happened: Thus the violence of the frost was in part the cause that the river Seine was not frozen.

The abbé Nollet on the contrary thinks, that the cold itself, to use the beautiful expression of Virgil,

—cursus frenaret aquarum;

And that

Concreſcunt ſubitæ currenti in flumine cruſtæ.

Vide Vol. I, pag. 402.

The reason which the abbé assigns in conformity to his hypothesis, why it sometimes happens that the river Seine and other rivers are not intirely frozen in the severest frosts, is this, that in such sudden frosts, when the ice of the edges is fast bound, there is not a sufficient quantity of pieces of ice floating in the stream to join together and form a continued surface.

This south wind so cold must naturally induce us to conceive what in reality happened in those countries, which lye south in respect of us ; for the sea froze in some places of the coast of Provence, and the greatest part of the fruit-trees were destroyed, as well as about Paris.

I had never seen the thermometer so low as this year. I find indeed in my registers that on the 6th of February 1695, the thermometer had fallen to 7 degrees in the same place where it now stands, and the cold of that winter was considered as the greatest that had been for a long time ; but it is not to be compared with that of the present year.

The winter continued likewise a long while, for on the 13th of March it froze still very hard, the thermometer being at 24 degrees.

We find in Mezerai's history of France, that the winter of the year 1608 was very long and very severe, and that the greatest part of the young trees were frozen ; yet that year was remarkably plentiful : If we may form a judgment from plenty at that time, and from the loss of trees at present, the last winter must have surpassed it.

The thermometer at the *highest* was at 63 degrees on the 11th of August about half an hour after 4 in the morning, and at 75 about 3 of the clock in the afternoon.

The barometer was at 28 inches 3 lines $\frac{1}{2}$ on the 19th of January, with a calm and serene

rene sky ; which was about the time of the severest frost : And on the 31st of December following it was at 28 inches 3 lines $\frac{1}{2}$, with a very thick fog and a calm. The barometer at the lowest was 26 inches 7 lines $\frac{1}{2}$, with a south wind and a small rain, on the 16th of December. The difference therefore between the greatest and the least height of the barometer was 1 inch 8 lines, which is a little more than the difference that is observed *here* at a *medium*, which is 1 inch and 6 lines.

The declination of the needle was on the 24th of December 10 degrees 30 minutes towards the west : From whence it appears, that the declination increases continually every year pretty much in the same proportion.

The quantity of rain that fell this year at Pont-briand near St. Malo was 18 inches 9 lines, which is less than what fell at Paris, and is very extraordinary ; for we had observed in the preceding years, that it rains much less at Paris than in that country which is bordering on the sea.

The great frost began some days sooner *there* than at Paris : It snowed *there* at the same time that the frost began with us, and was attended with a north-west wind ; whereas at Paris there was scarce any wind, and it was southerly.

The quantity of rain that fell this year at Zurich in Switzerland was 32 inches 6 lines

$\frac{1}{2}$; which confirms the account we have given before of the greater quantity of rain that falls in mountainous countries.

Anno
1710.

15 inches 8 lines $\frac{3}{4}$.

By which it appears, that this hath been one of the dryest years which we have had a great while; yet it hath been very abundant in grain, for the reason we have mentioned before, that the greatest part of the lands are cool and moist.

The thermometer marked the greatest cold of the whole year on the 11th of January, having fallen to 14 degrees $\frac{1}{2}$; but on the 12th it rose again to 27, which was its station on the 10th, and from that time the cold was but moderate.

The greatest heat was marked by the thermometer at 61 degrees on the 3d of August at sun-rising, and at half an hour after 2 in the afternoon it rose to 71 $\frac{1}{2}$.

My barometer at the *highest* was at 28 inches 3 lines $\frac{1}{2}$ on the 3d of January, with a south wind; which is very extraordinary: It was at the *lowest* on the 7th of March at 26 inches 10 lines $\frac{3}{8}$, with a south wind and rain. The difference therefore between the highest and lowest is 1 inch 4 lines $\frac{1}{3}$, which is a little less than the usual difference of 1 inch 6 lines.

On the 30th of December I observed the declination of the needle to be 10 degrees 50 minutes towards the west.

By

By the observations of Mr. Scheuchzer at Zurich the quantity of rain that fell was 23 inches $\frac{3}{4}$.

By which it appears that it was *there* likewise an uncommonly dry year.

The *greatest* height of his barometer was 26 inches 9 lines $\frac{3}{4}$ on the 3d of January; and the *least* 26 inches 0 line $\frac{1}{2}$ on the 25th of December.

25 inches 2 lines.

Anno

Which is much more than falls in a year ¹⁷¹¹ at a *medium*, which we have observed to be about 19 inches.

It is generally observed that a dry year is succeeded by a wet, and the comparison that Mr. Pope makes between the moral and the natural world is extremely just and beautiful.

Extremes in nature equal good produce,
Extremes in man concur to gen'ral use.

Ask we what makes one keep, and one bestow?

That power, who bids the ocean ebb and flow;

Bids seed-time, harvest, equal course maintain

Thro' reconcil'd extremes of drought and rain.

It is very extraordinary that this year should have given so much water, altho' it rained but very little during the two months of June and

and August, which together with the month of July very often furnish *as much* as the nine other months of the year; but the great snows in the month of February with the rains that followed them towards the end of that month, gave all at once a great quantity of water, (*namely*, 51 lines $\frac{1}{3}$) and occasioned a considerable overflowing of the river. The 28th, 29th, and 30th of July furnished about 31 lines of water, which was the greatest continued rain throughout the whole year.

The greatest height of the thermometer was 62 degrees $\frac{1}{2}$ on the 16th of June at sunrise, and at 2 of the clock in the afternoon it was at 73 $\frac{1}{2}$. It fell the lowest on the 15th of February to 20 degrees, but it rose again 2 days after to 36.

My barometer at the *highest* was 28 inches 5 lines on the 21st of January; and at the *lowest* 26 inches 9 lines $\frac{1}{3}$ on the 10th of December, with a very violent hurricane, the wind being towards the south, but with little rain. Thus the difference between the highest and the lowest was 1 inch 7 lines $\frac{2}{3}$; which is a little more than usual.

On the 30th of December I found the declination of the needle to be 10 degrees 50 minutes to the west, which is the same as in the preceding year.

The quantity of rain that fell at Zurich was 45 inches 1 line, Paris measure.

The

The *greatest* height of the barometer at Zurich was on the 22d of December at 27 inches; on the same day in the morning at Paris it was at 28 inches 3 lines $\frac{1}{4}$. The *least* height of the barometer at Zurich was on the 9th of February at 25 inches 11 lines $\frac{1}{2}$, and on the same day at Paris at 26 inches 11 lines $\frac{1}{2}$: But those were not the days of the greatest rise and fall of the mercury at Paris.

21 inches 2 lines $\frac{1}{4}$.

Anno

My thermometer fell the lowest on the last day of the year to 24 degrees $\frac{3}{4}$; and rose the highest on the 16th of August to 64, at sun-rising, and at about 2 in the afternoon to 76.

The barometer was at the highest on the 10th of February, and continued for several days, at 28 inches 4 lines $\frac{2}{3}$, the sky extremely serene, and a very gentle northerly wind: And I observed, that every time the barometer was above 28 inches, which happened pretty often in the course of this year, the wind was towards the north and east, and sometimes attended with fogs.

I have another barometer in which the mercury is always higher by 3 lines than in that which I commonly make use of.

My common barometer was at the lowest on the 6th of November at 26 inches 10 lines $\frac{2}{3}$, the sky serene with a moderate wind at east; but the mercury rose again very soon, and the wind shifted towards the west and south-

south-west. The difference between the highest and the lowest in the barometer this year was, as it is most commonly, 1 inch 6 lines.

Nothing remarkable in the winds this year. The declination of the needle was 11 degrees 15 minutes on the 30th of December.

Although this year was so favourable in outward appearance, yet it was extremely fatal by a raging epidemic measles. Mr. De Voltaire observes in his history of the age of Lewis XIV. that more than 500 persons died of it in less than a month at Paris; it spread throughout the kingdom of France, and almost extinguished the whole family of Lewis XIV. It is very remarkable that the great havock made by this disease was in the month of February, when there was such a serene sky, with very gentle northerly winds.

Anno
1713.

20 inches 7 lines $\frac{1}{2}$.

The month of July alone gave almost a quarter part of the whole, there having fallen 60 lines $\frac{7}{8}$.

The greatest cold of this year was on the 15th of January, when my thermometer sunk to 18 degrees and an half. The winter was severe, and it continued a frost throughout the whole month of March; but no snow fell in the course of the year.

The

The heat of this year was not great, for the thermometer rose but very little above 48 degrees, which is the mark of the *mean* state of the warmth of the air, such as it is constantly at the bottom of the quarries of the observatory: In the last days of August indeed the thermometer rose to 58 degrees.

There were a great number of fogs towards the end of the year, and the want of a sufficient heat in the month of August, prevented the fruits from having a perfect maturity.

The prevailing winds of this year were north and east; for which reason the heat was not great nor of long continuance.

The highest state of the barometer was 28 inches 4 lines $\frac{1}{2}$ on the 26th of November, with a slack wind from the north which continued several days both before and after, and no rain: On the 29th of October the barometer was in its lowest state at 26 inches 10 lines $\frac{1}{3}$, the wind being at that time southerly, but with little rain.

In the month of July, when it rained in two days near 25 lines, the barometer was at 27 inches 3 and 4 lines. The difference therefore between the highest and the lowest state of the barometer was, as usual, pretty nearly 1 inch and 6 lines.

On the 29th of December the declination of the needle was 11 degrees 12 minutes to the west.

Anno
1714.

14 inches 9 lines $\frac{1}{4}$.

This year may be called a very dry one, for the three months of June, July, and August furnished no more than about 6 inches; which months very often produce as much as the rest of the whole year: But then it is commonly by storms; and those sorts of rains do not contribute much to the fertility of the earth, but run off almost as soon as they fall, and do not penetrate into the ground,

*Sæpe etiam immensum cælo venit agmen
aquarum,*

*Et fædam glomerant tempestatem imbris
atris*

*Collectæ ex alto nubes: ruit arduus æther,
Et pluviâ ingenti sata læta boumque labo-
res*

*Diluit; implentur fossæ, et cava flumina
crescunt*

*Cum sonitu, fervetque fretis spirantibus
æquor.*

I am persuaded that the fogs, which are formed by vapours and exhalations, are much more useful than the rains for the nourishment of plants: And as there were many very thick fogs during the whole year, the harvest was very plentiful, and the fruits extremely well ripened.

My thermometer sunk the lowest on the 5th of February to 20 degrees $\frac{1}{4}$, but rose again

again very considerably in a little time. It was mounted up to 64 degrees on the 10th of July at sun-rising, and at half an hour after 2 of the same day it rose to 74.

There were violent winds in several seasons of this year, but they did not occasion so much disorder *here* as on the coasts of England and Flanders. The winds of this year were very variable, but the ruling wind was from the north.

The mercury was highest on the 7th of December at 28 inches 5 lines, with calm weather and a fog; and it was at the lowest the 9th and 10th of May at 27 inches $1\frac{1}{2}$ line: The difference therefore between the two stations was 15 lines $\frac{1}{2}$, and it usually is of 18.

The declination of the needle at the end of December was 11 degrees 30 minutes towards the west.

17 inches 6 lines $\frac{1}{2}$.

With regard to the thermometer Mr. De^{Anno 1715.} la Hire hath observed through a long succession of years, that it never changes its height in the caverns or quarries of the observatory, which are 14 fathom deep in the earth, but continues *there* at all seasons of the year at 48 degrees.

The thermometer was the lowest this year on the 18th of January at 18 degrees: It rose the highest on the 2d of July to 64, and at the same time there was a very considerable

derable storm; in the afternoon of the same day between 2 and 3 of the clock it got up to 76.

The mercury rose the highest in the barometer on the 22d of January to 28 inches 3 lines $\frac{1}{2}$, the wind being moderate at S. E, and there was a very thick fog. The first of December following it was likewise at 28 inches and nearly 3 lines, with a moderate north wind, and on the 6th of the same month it fell to 26 inches 9 lines $\frac{1}{2}$, the wind being moderate at S. S. W. Thus there was the usual difference of height in the mercury of 1 inch 6 lines.

I am persuaded that fine weather and rainy weather do not depend on the weight and lightness of the air, but that they are occasioned only by the wind; I do not mean the *wind* in general, but such winds as come from a distance from the north and south, and occupy the greatest part of the height of the atmosphere, and not such as are produced near the surface of the earth; for the sun raising more vapours in the southern countries than in the northern, the southern winds must give more commonly rain than the northern: And as we know by all the observations which have been made towards the north, that the atmosphere *there* is higher than towards the equator, it must happen, that the winds which shall blow from the north will cause the atmosphere to rise in our temperate

temperate zone more than usual, and by consequence the mercury will be raised by the greater weight of the atmosphere, and the air will become serene in consequence of such a wind. The contrary must happen with regard to the winds which shall blow from the south upon these countries.

We have examined the variation of the compass this year with three different needles, and of different construction; of which *two* are eight inches long; the one of *these* is what I have made use of for a great number of years: And we have found the variation to be 11 degrees 10 minutes. This declination is a little less than that of the preceding year. The *third* needle is 13 inches $\frac{1}{2}$ long, and it hath given us the same declination as the two others.

14 inches 4 lines $\frac{1}{4}$.

As there fell but little rain in the spring and summer, the hay and corn that is sown in March have yielded little, and the greatest part of the fruits have dried away without ripening. The wheat harvest however hath been very good, for the dryness which hath occasioned mischief on one side, hath been useful on another; the grain being clear from weeds which commonly choak it in rainy years.

It snowed very considerably during the whole month of January; the greatest snow was on the 31st of that month, which furnished

nished 16 lines depth of water, when melted.

The prodigious quantity of very thick fogs this year hath greatly contributed to the support of plants and trees.

The winds have been, as usual, extremely variable; but on the 31st of October there was a very violent wind from the south, and a rain of near 6 lines. On the last day of November the south-west wind was likewise very high, but without rain.

My thermometer at the lowest was sunk to 4 degrees $\frac{2}{3}$ on the 22d of January, which marks an excessive cold, but it lasted no longer than one night, for the following days the thermometer rose very considerably; and in the beginning of February, when the cold is very often extremely violent, it scarcely froze. In the severe winter of 1709, which hath been thought the coldest that had been ever known in these countries, this same thermometer fell no lower than 5 degrees.

The thermometer was very high at the end of the month of July, and through the whole month of August; and at the highest on the 23d of August at 63 degrees about sun-rising, with a moderate wind at S. W.

My barometer was at the lowest on the 1st of January at 26 inches 9 lines $\frac{1}{2}$ with a moderate south wind; and it rose the highest on the 16th of February to 28 inches 3 lines, with a north-east wind: The difference therefore

fore was 17 lines $\frac{1}{2}$, which is pretty near to what it usually is.

Having examined the variation of the compass with the three needles, which we mentioned in the preceding year, we found the two needles of 8 inches in length to give the declination of 12 degrees 20 minutes towards the west; and that of 13 inches $\frac{1}{2}$ to give 11 degrees 45 minutes.

This year was remarkable in France and England for the first appearance of the northern lights.

17 inches 8 lines $\frac{1}{2}$.

Anno

Which is somewhat less than what falls ¹⁷¹⁷.
communibus annis, which we have found to be 19 inches.

There were a great number of fogs in the course of this year, which have supplied the want of rains and snow.

On the 4th of July there was a violent storm with very large hail, for there were some hail-stones 7 lines in diameter, which did a great deal of mischief to the trees and fruits where the cloud passed.

The most common, and the most violent winds of these countries, come almost always from the south-west, and they very often bring rain; because there rise more vapours from the sea to load these winds, than those which blow from the east: And we always observe, that clouds increase considerably the force of the wind; the reason may

be this, that the wind, tho' in itself moderate, being confined between the clouds and the earth, its velocity must in consequence be augmented.

The thermometer was the lowest this year on the 13th of February, at 24 degrees; and it rose the highest on the 1st of August to 65 at sun-rising, and at about half an hour after 2 in the afternoon to 78.

The highest rise of the barometer was to 28 inches, 2 lines, $\frac{1}{8}$, on the 10th of February, and on the 29th of December following, with no very clear weather at either time: It fell the lowest to 26 inches, 10 lines, $\frac{1}{3}$, on the 22d of February, and on the 12th of March; there was a moderate rain in February, and but a little in March at those times. The difference between the highest and the lowest state of the barometer was 1 inch, 5 lines, and $\frac{5}{8}$, just bordering on the usual difference of 1 inch, and 6 lines.

The greatest, and the most continued winds of this year were in December, at which time there fell a great quantity of rain, altho' the barometer was only in a middle state.

We have remarked often, that the winds in the upper region of the air, as might be seen by the motion of the clouds, are very different from those near the surface of the earth, and which Lucretius calls,

—*Verrentes æquora venti.*

The

The declination of the needle on the 29th of December was 12 degrees 40 minutes towards the west.

13 inches, 1 line, $\frac{3}{4}$.

Anno
1718.

The present year hath been the driest we have had for 30 years, except the year 1694, when there fell in rain but 11 inches 9 lines $\frac{1}{2}$.

The Meteorological register continued by Mr. De la Hire the son.

The great drought of this year hath not hindered it from being very plentiful; not that there hath fallen much snow, for being reduced to water it yielded no more than 4 lines $\frac{1}{2}$; but the reason is, that the rains fell precisely at the times that the earth wanted them,

Tum pater omnipotens fecundis imbribus æther

Conjugis in gremium lætæ descendit, et omnes Magnus alit, magno commixtus corpore, fœtus.

infomuch that it may be said that not a drop of rain hath fallen uselessly through the whole year.

By considering the result of each month, we find that there rained almost an half more in the spring and summer, than what rained in autumn and winter; for there fell in the spring and summer 7 inches 7 lines; and in the autumn and winter 5 inches 2 lines.

The winds have been, as usual, very variable: The thermometer sunk the lowest on

the 10th of February to 21 degrees $\frac{1}{2}$; which is not the mark of excessive cold. It rose the highest at sun-rising, on the 22d of August to 70 degrees, and about three of the clock in the afternoon of the same day to 82 degrees; it was at the same height in the afternoon on the 11th, 21st, and 23d of that month.

One may consider the heat of 1718 as the greatest we had observed at Paris; not but that the same thermometer had in like manner rose to 82 in the years 1706, 1707, and 1709; but then it rose to that point but *once* in each of those years; and in 1718 it rose to that height *four* different days, of which *three* were successive, and it is that *continued* heat, altho' at the same degree, which makes us feel its power.

The barometer fell the lowest on the 11th of January, to 27 inches, with a south wind: It rose the highest to 28 inches, 4 lines, 2 points, on the 15th of February, with a calm; but afterwards a gentle wind sprung up from N. E.

We observed the declination of the needle on the 30th, and 31st of December, to be 11 degrees 30 minutes to the north-west.

Anno

1719.

The Meteorological observations continued by Mr. Maraldi.

9 inches, 4 lines, $\frac{1}{3}$

By which it appears that this year hath been extremely dry, not having half the quantity of rain that falls *communibus annis*, which hath been found to be 19 inches.

During

During the three months of March, April, and May, there fell no more than one inch; which is the cause that the crops of grain, sown in those two first months, have been extremely small.

During the month of January the thermometer hath been most commonly above 35 degrees; which marks a very temperate state of air: On the 2d of January *indeed* it fell to 27 degrees $\frac{1}{3}$, with a north-west wind, and a little snow.

In February, and a great part of March, it was almost always above 40; but on the 28th of the last mentioned month it sunk to 27 degrees $\frac{2}{3}$, which is near the height it had in the beginning of January, with a wind N. N. E. and on the 29th it fell still lower; that is to say, to 26 degrees $\frac{1}{3}$.

The mild air that prevailed in February and in March, caused the greatest part of fruit trees to be in blossom, but a great part of those blossoms were blighted by the cold at the end of March, and by the north wind which blew almost the whole month of April.

This temperate air hath been still more sensible in the southern parts of France and Italy, where it hath produced uncommon effects, such as ripe fruits in the depth of winter,

— *Atque alienis mensibus æstas.*

The thermometer rose the *highest* on the 16th of July, it being that day at 4 o'clock in the morning at 69 degrees; and at 3 o'clock in the afternoon it mounted up to 82 and $\frac{1}{2}$, with a wind S. S. E. In the greatest heats of preceding years this thermometer had never reached beyond 82.

The thermometer continued very high through the month of July, and in the beginning of August; on the 7th of that month it was at 3 o'clock in the afternoon at 82, with a wind S. S. E. Thus the heat of this year was not only very great, but of long continuance, it having lasted from the beginning of June, to about the middle of September.

The marquis Salvago hath written to me from Genoa, that his thermometer rose the highest on the 7th of August, which was the day likewise that it rose very high at Paris, but not so high as on the 16th of July by $\frac{1}{2}$ of a degree; he adds that in 1718 his thermometer was the highest on the 11th of August, which was one of the days likewise of the greatest heat at Paris, and that it rose the present year two degrees higher than in 1718.

By the observations of Father Feuillée at Marseilles, his thermometer was at the highest on the 15th of August, eight days after one of the hottest days at Paris.

The *lowest* that the thermometer hath fallen this year, was on the 28th of December

to 24 degrees, which marks only moderate cold.

Some have imagined that the greater, or less heat, which glows in the same season in different years, may arise from the spots that are observed at the same time in the sun; and as when he is spotted he doth not throw forth so great a number of his rays to the earth, that the heats must of consequence be less than when he hath no spots. But the experience we have had the two preceding years shews, that this explanation is not sufficient; for during these same years 1718, and 1719, we have rarely seen the sun without spots; and sometimes there were so great a number at once, that we have counted 9 or 10 at the same time, the greatest part of which were very large: Yet notwithstanding so many spots during the two last years, the heat hath been remarkably violent. Thus it appears that the different temperature of the same seasons in different years, cannot be attributed to the spots of the sun, but may more probably be owing to the different exhalations of the earth, sometimes colder and sometimes hotter, and to the diversity of winds which rule in the same season, and which have not, as far as we know at present, any regular periods in our climates.

The barometer was on the 24th of January at 28 inches 4 lines, which is the highest that it rose, the sky serene and the air calm:

It

It was again at the same height, bating one line, on the 22d, 23d, and 24th of December following, with the sky cloudy, but the wind very still.

On the 8th of January it was at 26 inches 8 lines, with a violent south wind and rain; and on the 9th of January it was at 26 inches 9 lines, the sky serene. It rose afterwards from the 9th to the 12th; but the 13th and 14th it fell again to 26 inches 10 lines, with a wind W. S. W. and a little rain. The barometer was likewise on the same day, *namely*, the 14th of January, very low at Genoa, being at 26 inches 0 line $\frac{1}{4}$, with a very great north west wind.

In the night between the 16th and 17th of January, there was at Paris a furious west wind, which did a great deal of mischief; the barometer was at the same time at 27 inches 2 lines. The mercury was very low on the 20th and 21st of February, being at 26 inches 7 lines, with changeable weather. At Genoa the barometer was likewise very low, on the 21st of that month at 26 inches 11 lines $\frac{1}{2}$, with a calm. Thus the barometer was low on the same days at Paris, and at Genoa, with this difference, that it was higher at Genoa by about 4 lines $\frac{1}{2}$ than at Paris, because Genoa is situated on the sea-shore, and the observatory is elevated above the sea about 300 feet. On the 26th of September I observed the variation of the
I compass

compass with a needle of 8 inches. The declination was 12 degrees 30 minutes north-west: I observed it again on the 30th of December, and it appeared to be a little less; as if it had *diminished* between the month of September, and the conclusion of the year.

According to the report of Mr. Picard, the needle had no declination at Paris in the year 1666. From that time it hath declined from the north towards the north-west, and this declination hath been always increasing on the same side for the space of 41 years, that is to say, to 1717 when we observed it to be 12 degrees 45 minutes north-west. In 1718 we observed it to be 12 degrees 30 minutes, as if it had *diminished*. In 1719 it appeared to us still 12 degrees 30 minutes, as if it had been *stationary* these two last years: And by the last observation made at the end of December, it hath appeared to be *retrograde*.

In the proportion of 12 degrees 45 minutes change of declination in the space of 41 years, there results for each year a variation of 18 minutes.

“ The needle at London declined to the
 “ *eastward* 11 degrees 15 minutes in the year
 “ 1580. In 1657 there was no variation,
 “ the needle then pointing due north. In
 “ 1672 the variation was observed by the
 “ late Dr. Halley 2 degrees 30 minutes to-
 “ wards the *west*, and in 1692, 6 degrees.
 And

“ And towards the beginning of the year
 “ 1723, it was found by Mr. Graham, from
 “ the medium of a vast number of observa-
 “ tions, to be then 14 degrees 17 minutes
 “ the same way.”

*But by the observations of the Academy at
 Paris on the 4th of January 1723 it was only
 13 degrees; and had continued at the same
 point for three years successively.*

Anno
 1720.

17 inches and almost 2 lines.

Which is 2 inches less than what falls
communibus annis. The waters of a great
 number of fountains have been observed to
 diminish within these three years; and some
 springs have been intirely dried up.

There was this year, through the whole
 kingdom almost, a great abundance of fruits
 of all sorts, of corn and forage.

The barometer hath kept commonly at a
 great height during the whole year. On the
 8th and 15th of January, the 10th of July,
 the 3d of September, and the 1st of October,
 it rose to 28 inches 2 lines, the sky some-
 times fair, and sometimes covered with fogs.

The barometer was 27 inches 1 line, which
 is the lowest state it hath been this year, on
 the 13th of February after a violent wind
 from the west and rain, on the 27th of
 March with rainy weather and a very high
 wind from south-west, on the 19th of April
 with a cloudy sky, on the 20th of October
 with

with a still air and gloomy weather, and on the 20th of December with an high wind from S. S. W.

There were a great many storms of winds in the course of this year.

The greatest cold of this year did not cause the thermometer to sink below the 30th degree.

In the greatest heat of the year, which was on the 20th of July, the thermometer rose only to 77 degrees at 3 o'clock in the afternoon, with a wind S. S. E. and fine weather.

We observed in the beginning of September of this year the variation of the compass to be 13 degrees north-west; the needle made use of was 8 inches long.

12 inches 7 lines $\frac{2}{3}$.

Anno

1721.

For these 32 years that we have observed regularly the quantity of rain, there have been only two years more dry than the present, viz. the year 1719 in which there fell only 9 inches 4 lines, and the year 1694, which yielded 11 inches 9 lines; all the rest have given more rain: yet had it not been for these observations which are made with care, the present year would have been thought one of the most plentiful in rain. It is true that it rained often, but as the rain was fine and small, it yielded but little water. Notwithstanding the dryness of this year, there hath been a plentiful crop of all sorts
of

of grain, because the rain, tho' little in quantity, hath fallen in proper seasons for the fertility of the country; besides that, the clouds, which during the course of this year have very often over-cast the sky, have not permitted the rays of the sun to dry and burn up the ground.

Although the weather hath been lowring, and the rains frequent, yet the barometer hath kept most commonly at a great height; it was at 28 inches 6 lines, which was the highest that it rose, on the 18th and 20th of January, the air being on those days still, and the sky cloudy: It was at 27 inches 2 lines, which is the lowest that it fell, on the 3d of November, with a south-west wind, and rainy weather.

The greatest cold of the year was marked by the thermometer on the 22d of February at 19 degrees, the earth being covered with snow.

The hottest days were the 7th and 8th of August, the thermometer having risen on the last mentioned day to 72 degrees at 3 o'clock in the afternoon, with a south and south-east wind, and the sky somewhat cloudy; it rose again to the same height of 72 degrees on the 7th and 28th of September, the sky serene, and a south-east wind the preceding day.

We have several times observed, that the greatest heats of summer have happened at the

the times that the ruling wind hath been south-east.

We have remarked likewise that water hath not always froze at the same degree of the thermometer, but it appears to us, that there is some diversity dependent on the different state of the air; for example, on the 14th of October, the sky being serene, the air still, and the thermometer at 35 degrees, the water froze in the court of the observatory. It froze again still harder the 15th of December, the sky being serene, the air calm, and the thermometer at 30 degrees: yet on the 1st and 2d of January in this year, the air being calm but mixed with thick fogs, and the thermometer at 30 degrees, as it was on the 15th of December, and 5 degrees lower than on the 14th of October, the water did not freeze. There was no other difference in these circumstances, but that the sky was serene when the water froze, and that it did not freeze when the sky was over-cast, and the air loaded with vapours.

The compass continued stationary at 13 degrees north-west.

14 inches 6 lines $\frac{1}{2}$.

Anno

1722.

The rain therefore of this year hath been much below the rate of common years, which hath been determined to be 19 inches: The same thing hath happened for eight years successively; no wonder therefore that the springs

springs of water have diminished greatly for several years.

Although the quantity of rain this year hath been greater than that of preceding years, yet there hath not been so great an abundance of corn; which may be owing in part to the rain not having fallen in the most proper intervals of time for the fertility of the earth.

It is perhaps a thing without example in this climate, that a whole month passed without a drop of rain, as it hath happened in the month of October of this year; which hath made the autumn extremely agreeable.

Every Englishman I imagine must exult with the reflection that he hath often enjoyed this pleasure in his own country, which the Frenchman treats as a miracle: And it justifies the observation of king James II. quoted by Sir William Temple, " That he thought that was " the best climate where he could be abroad " in the air with pleasure, or at least without trouble and inconvenience, the most " days of the year, and the most hours of the " day; and this he thought he could be in " England, more than in any country he " knew of in Europe.

The greatest height of the barometer was 28 inches 7 lines $\frac{1}{4}$, and it continued at this height for four days successively in the month of January, from the 2d to the 5th inclusive.

The

The sky was at that time over-cast with a great fog, and the air very calm. This is the greatest height that the mercury hath risen in our climate, ever since the Academy begun to make observations.

The barometer was the lowest on the 31st of December at 27 inches 1 line $\frac{1}{2}$, with a violent south-west wind.

The greatest cold of this year marked by the thermometer was on the 24th of January, it having fallen to 25 degrees.

In the greatest heats of this year, the same thermometer rose to 72 degrees at 3 of the clock in the afternoon on the 15th 16th and 27th of June. The wind was on those days south or south-east.

The compass still stationary at 13 degrees north-west.

Mr. De Montagnac consul at Lisbon hath sent an account that they were very much surpris'd in the Province des Algarves to see there in December 1722, and in the month of January following, trees green and in blossom as in the spring, plums and pears as ripe and good as in the month of June; figs as large as in April and May, and vines that had already four grapes. The great variety of the same seasons must sometimes bring such sort of wonders, and it is proper to remark the extremes and bounds of this variety.

N. B. The quantity of rain that fell at Bergues near Dunkirk in 1722, was 24 inches 11 lines $\frac{1}{2}$.

Anno
1723.

7 inches 8 lines. Which marks an extreme drought.

The rains that fall in March, April, and May, contribute much to render the earth fertile in corn and forage: And thus through the timely rain of those three months, altho' it did not exceed an inch and a line, the crops of grain were moderate, and that of forage still better.

The great drought hath not been general through the whole kingdom, for towards the southern parts there were very copious rains in the spring.

The thermometer fell on the 10th of February to 17 degrees, which was the lowest of the whole year.

The heat of this year was not great, tho' it lasted a long while. The highest that the thermometer rose was to 75 degrees on the 20th of August and the 5th of September at 3 of the clock in the afternoon, with a south-east wind.

The barometer rose often to 28 inches 4 lines, with a sky sometimes serene, sometimes cloudy, and sometimes foggy. The barometer was the lowest on the 22d of December at 27 inches 3 lines, with rainy weather, and a tempestuous south-west wind.
In

In the morning of the 23d, the wind having ceased, the barometer was risen to 28 inches; so that in 24 hours it rose 9 lines.

The compass hath continued at the same point of variation, *namely*, 13 degrees north-west, ever since the year 1720.

It was commonly thought in 1723, that the Seine was lower at Paris than it had been in 1719, and indeed the year 1723, in which there fell but 7 inches 8 lines of rain-water, was a drier year than 1719, when there fell 9 inches 4 lines. Mr. Delisle the younger was willing to be assured of the truth, and consulted the water-marks that are in different parts of the river: He found that at the end of October, and in the beginning of November, which was the time that the river was at the lowest, it was still about 3 inches higher than it had been in the summer of 1719. The consequence would seem very just from a greater drought, that there was a less depth of water.

At Bergues near Dunkirk the quantity of rain in 1723 was 23 inches 0 line $\frac{8}{9}$.

12 inches 4 lines

Anno

From whence it appears that this year also ^{1724.} hath been a dry one.

On the 26th of November the thermometer was at 28 degrees, which was the lowest that it fell this year, and where it continued but one day: Thus the greatest cold of this year, which hath been extremely mo-

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derate,

derate, was near the end of November, the wind being north-east.

As to the great heats of this year, they began in the month of June and lasted a long while. The thermometer was almost always above 50 degrees at sun-rising the whole month of June; and during the months of July, August, and a part of September, it kept between 53 and 60. On the 3d of July at 3 of the clock in the afternoon it rose to 80 degrees, the wind south-east; on the 1st of September at the same hour it rose to 82, the wind again S. E.

The barometer was at 28 inches 4 lines the seven first days of January, which was the greatest height it rose this year, the sky was then cloudy, and the air calm. The 19th of December it was at 26 inches 4 lines $\frac{1}{2}$, which was the lowest that it fell. There was that day, and the preceding 18th, a furious south wind, which continued with the same violence for two days, and was attended with rain. Through all the observations we have made from the year 1696, we find only the year 1702 when the mercury fell to 26 inches 5 lines with a *moderate* south wind.

The south-east winds, which are rare in these climates, have prevailed more than usual in the year 1724.

According to the public news-papers it was with a south-east wind that the furious tempest happened on the 19th of November

in

in Portugal, which hath caused such ruins to towns and country where it passed. At Paris on the 19th of that month we had not a breath of air stirring, and the two preceding days the wind was E. N. E. and the two following it was N. E. Thus the wind that there was in Portugal was very different from the winds which we had about that time at Paris.

The needle still stationary at 13 degrees. The quantity of rain at Bergues near Dunkirk in 1724, was 25 inches 7 lines.

17 inches, 6 lines, $\frac{1}{3}$.

Anno
1725.

This year seemed to be extremely abundant with rain, for from the beginning of May to the end of the year, the rains have been very frequent, and few days have passed in a continued series without some rain falling; but the drops have most commonly been very small, and produced but little water.

The sky being almost always over-cast, and the frequency of the rains have been the cause that the year was very *late*, they did great mischief to the corn after it was cut, and prevented the grapes from ripening.

The thermometer fell no lower than 26 degrees, on the 23d and 26th of February, with a great fog and calm air. During the summer the highest degree it was risen in the morning was 60, which happened on the 5th of July, the 10th, the 11th, the

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12th,

12th, the 13th, and the 14th; on the same days in the afternoon at 3 o'clock it was mounted up to 70; and on the 13th at the same hour it rose to 76. The other days of July and August the thermometer was lower, which shews the heat to have been moderate, and of short continuance.

The barometer was very high from the end of January, to the beginning of March, during which time the sky was most commonly over-cast with fogs, without any rain; it fell 2 lines the 1st and 2d of March, with rainy weather; it rose the 3d and 4th of the same month to 28 inches 4 lines, with serene and calm weather, which was the greatest height it rose this year.

On the 18th of December in the morning there was a violent storm mixed with rain and thunder, then the barometer fell to 27 inches, and the following day the 19th it fell still lower 2 lines, being at 26 inches 10 lines; which was the lowest it fell this year. Thus the variation between the *highest* and the *lowest* was 1 inch and 6 lines.

The storm of the 18th of December did a great deal of mischief to the houses of Paris, and we have heard by private letters that it was felt very severely at Dijon.

On the 6th of November, and the 30th of December, the declination of the compass, observed with a needle four inches long, as in the preceding years, was 13 degrees 15
minutes

minutes north west. This year therefore the needle began again to advance towards the west, as it had done before, after having been stationary 5 years.

The quantity of rain at Bergues near Dunkirk was in 1725 22 inches 2 lines. These accounts from Bergues, and those from Zurich, which we mentioned formerly, confirm the observation, that there falls more rain near the sea and in mountainous countries, than in the midland parts.

In the great sea that lyes between our continent and America, there is commonly found no ice after the month of April on this side 67 and 68 degrees of north latitude; but Mr. Deslandes, who hath for some years resided at Brest, and keeps a correspondence with our principal colonies, hath heard that this year the ice was not melted in the month of June; and that the French ships which go out for the cod-fish, met mountains, and floating islands of ice in 41 and 42 degrees of latitude. On the 15th of June two vessels had like to have been demolished by these pieces of ice at 45 degrees. It is possible that the cold, or the little heat, which hath been throughout Europe this summer, may be owing, at least in part, to this cause. The winds of south and south-west have continued a long time; and Mr. Deslandes assures us, that they have been constant in Bretagne, from the 3d of March to the 18th of July,

T 4

without

without ever turning to the north, which was very extraordinary. Those winds that should naturally have brought us warm vapours, were loaded only with particles detached from those vast *piles* of ice, which they found in their way *vagrant* from their season; and those particles came and dissolved *here* in abundant rains. The *meteors* of one country depend often on *those* of another, they are all *linked* together, however distant they may be.

Mr. Deslandes hath made a singular observation on the barometer; he hath two at Brest, and the mercury of both appeared continually fixed at 26 inches, 4 lines, from the 2d of February to the 1st of September, when it rose all of a sudden to 28 inches 2 lines, and varied afterwards as usual. A strange immobility for seven whole months, a kind of dead repose in the atmosphere of lower Bretagne! The rains there have been excessive, a perfect deluge that hath drowned every thing.

At Brest, and in several ports of Bretagne and Normandy, they have often observed, that at about a league from the coast the water of the sea was fresh, even fit for drinking, and of a colour different from the rest; which is the effect of continual and abundant rains. The same cause produced probably the sweetness of the waters of the port of Syracuse, which is mentioned by Pliny,

E/

*Est in exemplis, Dionysio Siciliae tyranno,
cum pulsus est ea potentia, accidisse prodigium,
ut uno die in portu dulcesceret mare.*

*And it is from some observations of the like
kind, I presume, that Virgil poetically prays,*

Sic tibi, cum fluctus subter labère Sicanos,
Doris amara suam non intermisceat undam.

11 inches 4 lines $\frac{2}{3}$.

Anno
1726.

For these 38 years since we began to measure the quantity of rain that falls at the observatory; we find but two years which have afforded less, *namely*, 1719 and 1723. The distribution through each month hath been very unequal; the *rains* of the spring which commonly fertilize the earth have been in small quantity; *those* of April did not exceed 7 lines, and *that* of May was only 2 lines. But the rains which came in June during the ripeness of the corn, and which amounted to 24 lines, have repaired in part the *scarcity* that the want of rain in the two preceding months, and the great heats which begun sooner than usual, would otherwise have occasioned.

The greatest cold marked by the thermometer was on the 19th and 21st of January; it fell those two days to 20 degrees at sun-rising, which is the mark of moderate cold, yet by letters from Montpellier, and Marseilles, we have advice that the cold *there* was

was excessive, altho' it did not last long, and that it had caused a great number of orange trees to perish.

The heats which began sooner than usual in this climate raised the liquor of the thermometer to 72 degrees, on the 31th of May at 3 in the afternoon. On the 1st of June at the same hour it rose to 75: All the rest of the months of June, July, and August, it kept at a great height; and on the 27th and 28th of August it got up to 77 degrees, which is the greatest height it rose this year. The heat having begun in the month of May, and continuing through the succeeding months, hath advanced the maturity of fruits near a month sooner than usual.

Father Laval writes from Toulon, that the heat there on the 14th of July was excessive, and that the thermometer of Mr. Amontons rose to 57 degrees 5 lines, higher by 5 lines than he had found it at St. Domingo, and Louisiana, in the summer of 1720.

Mr. Montvalon, counsellor of Parliament at Aix, being near Orange, with a thermometer regulated by that of the observatory, on the 13th of July at three in the afternoon, found the liquor at 90 degrees, with gloomy weather, and a south-east-wind: It rose therefore that day 13 degrees higher than in the greatest heat of this year at Paris, which happened on the 27th and 28th of August. The same Mr. Montvalon being
the

the 12th of August near Aix, observed the thermometer at 79, and on the 27th of the same month at 75, which is less by 2 degrees than ours was at the same time; from whence it appears that in Provence the greatest heats have been this year on the 13th and 14th of July, whereas at Paris we had them not till towards the end of August.

The barometer never sunk lower this year than 27 inches 2 lines, which happened on the 17th and 21st of December, with a south-west wind, and cloudy weather. During the greatest part of the year it kept very high; and on the 27th of December it rose to 28 inches 8 lines, with a gentle north wind, and cloudy weather. This was the greatest height that we had ever seen in the barometer.

The declination of the needle was on the 5th of December 13 degrees 45 minutes north-west.

13 inches 8 lines.

Anno
1727.

Notwithstanding the drought of this year, and the long continuance of the heat, there was in this climate an abundant harvest; because the rains fell at the most proper times; and those of the month of May, June, and July, which contribute most to fertilize the fields, were extremely copious.

The thermometer fell the lowest on the 7th of February to 28 degrees, which is the mark of a very moderate cold: The wind
was

was *then* south, and it is remarkable, that the thermometer was lower than the two preceding days, when the wind was north and N. W. This falling of the thermometer with a south wind is owing, at least in part, to that wind's bringing back *at first*, by a kind of reflux in the atmosphere, the particles of a cold air which the north wind had driven to the south : But this same south wind having continued, *at length* causes the thermometer to rise, and the air to be temperate.

For the like reason, when after a south wind that of the north begins to blow, it makes the thermometer to rise ; but depresses it after some time. The same thing happens with regard to our sensation, which is more quick and sudden than the motion of the liquor in the thermometer, that we find sometimes the winds from the north temperate, and those from the south chilling.

From the 7th of February the thermometer continued to rise considerably in the following months, till on the 10th of May, having been in the morning at sun-rising at 56 degrees, it rose at 2 in the afternoon to 70 ; which is an height it very rarely arrives at in this month. It continued to be at a great elevation all the rest of the month of May, through June, July, and at last the 7th of August at 3 in the afternoon it mounted up to 80. In all the hottest days the wind was south or south-east, as it commonly

ly is in excessive hot weather. The thermometer was pretty high the rest of August and in September; thus the heats having begun in May, and not ending till September, have lasted 5 months; which is not common in our climate.

There was during almost the whole year a great number of *spots* in the sun, and sometimes *bigger* than the surface of the earth; which did not hinder the heat from being violent.

The barometer kept at a great height almost the whole year, it rose to 28 inches 4 lines the 1st of December; and it fell to 27 inches 1 line the 28th of the same month. There was no violent wind but in the night between the 4th and 5th of January, which lasted only that night.

The declination of the needle observed the 3d of January 1728 with the common compass of 4 inches, was found to be 14 degrees.

16 inches 1 line $\frac{1}{2}$.

Anno

Mr. De Montvalon, counsellor of parliament at Aix, hath communicated to us the observations he hath made at Aix in Provence, on the quantity of rain that fell there in 1728. By which it appears that it hath rained there 8 inches 8 lines more than at Paris.

The greatest heat of this year made the thermometer rise to 75 degrees on the 17th of July at 3 in the afternoon.

In

In the three first months of this year it was the lowest on the 12th and 13th of February at 26 degrees; to which point it fell again on the 29th of December following, on the 30th of the same month it fell to 23, and the 31st to 21.

The thermometer of Mr. De Montvalon, regulated by that of the observatory, was at Aix on the 8th of February at sun-rising at 28 degrees; there were therefore 2 degrees of difference between the greatest cold at Aix, which happened on the 8th of February, and that at Paris which was on the 12th and 13th of the same month.

The greatest heat was at Aix on the 17th and 18th of August at 81 degrees, the wind south-west and west; whereas our greatest heat, which was on the 17th of July, did not exceed 75, and the wind was east.

Mr. Weidler observes that the greatest heat of this year happened at Wirtemberg on the 29th of June, with a south-east wind.

The greatest cold at Wirtemberg was on the 26th of February, with a gentle wind from the east; at Paris it was on the 12th and 13th of the same month, with a north-west wind; and at Aix on the 8th with a wind from the same quarter: By which it appears, that in the more northern countries the great cold hath ceased *later* than in the southern, and that the great heats have happened there *sooner* than in the southern.

The barometer hath been very often in the course of the year above 28 inches; the lowest that it hath fallen was 27 inches; which happened on the 7th of December, the wind being south-west with rain. The mercury was at 28 inches 4 lines on the 8th of February, on the 10th, the 14th, the 15th, and the 16th of March, on the 13th, and 14th of August, and on the 12th of December; the air having been those days calm and serene, except on the 14th of March when it was cloudy, as it was some days before and after.

We have been thus particular that the reader may see the difference of the barometer at Wirtemberg and Paris. Mr. Weidler did not observe the mercury above 28 inches 2 lines, which happened on the 11th of March.

The winds which have been most frequent at Paris this year were those of the north, which cool the air; and those of the south and south-west, which bring us rain.

By the observations of Mr. De Montvalon it appears, that at Aix the winds are more regular than at Paris; that those which have been the ruling winds there, from the beginning of the year to the end of June, were almost always from the north-west, or from the opposite south-east; that it is the south-east wind, which comes to them from the Mediterranean, that brings them the rain. It appears likewise, that in July and August
it

it was the west wind which was of the longest duration ; this wind gave them their fine weather, the dryness, and the great heat which was felt on the 17th and 18th of August.

By the comparison of our observations with those of Mr. De Montvalon it appears, that the winds have been almost always different.

The declination of the needle observed on the 17th of November with the compass of the preceding years, was 13 degrees 50 minutes north-west, a little less than what we observed in the beginning of January this year, which was 14 degrees.

Mr. Weidler observed at Wirtemberg with a needle of 2 inches $\frac{1}{2}$ the declination to be 13 degrees north-west ; and in the month of June, in the observatory at Berlin, with a needle of 6 inches, he found the variation to be 12 degrees $\frac{1}{4}$.

Anno
1729.

17 inches 0 line $\frac{2}{8}$. At Aix in Provence,
18 inches 3 lines $\frac{2}{3}$.

The greatest cold marked by the thermometer at Paris, was on the 18th, 19th, and 20th of January ; the liquor descended on the 18th to 13 degrees, the 19th to 9 $\frac{1}{2}$, the 20th to 11, with a gentle north wind.

At Aix the greatest cold was on the 9th of January the thermometer having fallen to 17 degrees $\frac{1}{2}$. The greatest heat at Paris was on the 18th of June, when the thermometer

rose

rose at sun-rising to 63 degrees, and at 3 in the afternoon to 78.

At Aix the greatest heat was on the 20th of July, the thermometer having risen at 3 in the afternoon to 80 degrees.

At Paris the barometer rose no higher than 28 inches 4 lines $\frac{1}{2}$, which happened on the 6th of February, with a north-east wind, and a serene sky, and on the 9th of March, with the same wind. It fell the lowest to 27 inches 1 line $\frac{1}{2}$ on the 22d of February, with a south-west wind, and cloudy.

The greatest height of the barometer at Aix, was on the 31st of December at 27 inches 10 lines, after a great rain.

The least height at Aix, was on the 21st of November at 26 inches 11 lines.

The declination of the needle at Aix was 14 degrees. At Marseilles 14 degrees 50 minutes.

16 inches 0 line $\frac{1}{2}$. At Aix there fell no more than 11 inches 9 lines $\frac{5}{12}$. Anno 1730.

The greatest cold of the year at Paris was on the 20th and 27th of January, the thermometer having fallen the 20th to 24 degrees, and the 27th to 23, with a north-east wind.

At Aix the greatest cold was on the 22d of January, the thermometer having fallen with a north-east wind to 25 degrees.

At Paris the greatest heat was on the 5th of August, with a south-west wind, when

the thermometer was in the morning at 63 degrees, and rose at 3 o'clock in the afternoon to 76.

At Aix the greatest heat was on the 14th of August, the thermometer being at half an hour after two at 81 degrees $\frac{1}{4}$. The wind was then W. S. W. which is always warm in that country, and melts sooner the ice or snow in winter than the S. E.

The least height of the barometer at Paris was 27 inches 2 lines, on the 9th, the 10th, and 11th of March, the sky cloudy, with a gentle south-west wind. At Aix it was at its least height on the 9th of March, at 26 inches 8 lines $\frac{1}{2}$, on the 10th at 26 inches 9 lines $\frac{1}{4}$, on the 10th at 26 inches 9 lines.

The greatest height of the mercury at Paris was on the 22d of January, at 28 inches 5 lines, with serene weather, and a north wind; on the same day at Aix it was only at 27 inches 5 lines. The greatest height of the mercury at Aix was on the 31st of December, at 27 inches 10 lines: On that day at Paris it was at 28 inches 3 lines. At Marseilles the greatest height of the barometer was observed to be on the 31st of December, at 28 inches 2 lines $\frac{1}{4}$.

The least height was on the 11th of March at 27 inches 2 lines.

The barometer at Marseilles stands at the height of 144 feet above the level of the sea.

In

In order to reduce to the level of the sea the observations on the barometer that were made at Aix, which is only 4 leagues distant from the sea, Mr. de Montvalon went the 30th of May to the sea-shore, where he observed the barometer to be precisely 28 inches. The height was observed at the same time at Aix to be 27 inches 4 lines; the difference therefore was 8 lines, which according to the rules prescribed in the memoirs of the Academy for 1703 and 1705, would give the elevation of Aix above the level of the sea, at about 510 feet. At Paris, which is distant about 40 leagues from the sea, the elevation of the tower where the barometer is placed hath been computed only at about 270 feet; thus the height of the barometer *there* should be about 4 lines greater than at Aix; which reduces the difference of the observations on the 9th, 10th, and 11th of March to about a line.

On the 20th of November the declination of the needle at Paris, observed with a needle of 4 inches, was 14 degrees 25 minutes north-west.

N. B. *I have been the more particular in giving the meteorological observations at Aix, and Marseilles, relative to those of Paris; because as it is now the fashion to send persons in a declining state of health to the south of France, it may be of use to the physician to*

know the temperature of the air, the weight of the atmosphere, and other diversities compared with those of Paris, and in consequence of that with those of our own country.

Anno
1731.

10 inches 3 lines $\frac{1}{2}$.

Consequently this year was very dry; the crops of grain were very small, and that of forage still worse.

The winter was not extremely severe, but the cold lasted a long time. The liquor of the thermometer did not sink below 21 degrees.

The summer was very long, and the heat excessive; it made the liquor of the thermometer rise to 71 degrees on the 6th of July at sun-rising, but at three o'clock in the afternoon the liquor was mounted no higher than 74; whereas on the 10th and 11th of August, when we observed in the morning the liquor at 71, it rose to 82 at three in the afternoon.

The least elevation of the mercury in the barometer was 27 inches 1 line, with cloudy weather, and a north-west wind, on the 9th of February; and the greatest elevation was on the 10th, 11th, 12th, and 13th of June, at 28 inches 4 lines, with serene weather, and a north-east wind.

We enjoyed during the greatest part of this year a very serene sky; of which there
had

had been no example for a great number of years.

The river Seine was extremely low, and did not begin to increase till about the beginning of November.

On the 5th of December we observed with a needle of 4 inches the declination to be 14 degrees 45 minutes north-west.

13 inches 9 lines $\frac{1}{2}$.

Anno
1732.

The greatest cold was towards the end of January; on the 26th of that month the liquor of the thermometer, which is still the same that was made use of from the beginning of our observations, was at 19 degrees $\frac{3}{4}$.

The greatest heat was on the 30th of July, and the 2d of August; on those days the thermometer was at 66 degrees at sun-rising, and at 74 at 3 o'clock in the afternoon.

The barometer shewed the greatest rise of the mercury to 28 inches 5 lines on the 3d, 4th, 5th, 6th, and 7th, of December, with great fogs which lasted a considerable part of that month. The least height of the mercury was 27 inches 6 lines, on the 10th, 11th, and 12th of April, with cloudy weather, and a south-west wind.

On the 5th of September the declination of the needle was 15 degrees 15 minutes north-west.

9 inches 9 lines $\frac{1}{2}$.

1732.

The thermometer marked the greatest cold of this year on the 31st of January, and the 1st of February, at 26 degrees $\frac{1}{2}$.

The greatest heat of summer was on the 7th, 8th, and 9th of July. The liquor of the thermometer rose to 71 degrees at sun-rising, and to 77 in the afternoon.

The greatest height of the barometer was 28 inches 6 lines, on the 5th, 6th, 7th, and 8th of February, attended with great fogs.

The lowest state of the mercury was 27 inches 4 lines $\frac{1}{2}$, on the 30th of March, and the 2d of April, with cloudy weather, and a gentle south-west wind.

There were this year very high winds from south and south-west, and particularly in the months of January, March, and December.

I observed several times in the beginning of December, with the needle of 4 inches, the declination of the compass to be 15 degrees 45 minutes north-west.

17 inches 6 lines $\frac{1}{2}$.

Anno
1734.

The quantity of rain that fell this year at Utrecht, observed by Mr. Musschembroek, was 34 inches 9 lines; and the sum of evaporation in the whole year was 24 inches 2 lines $\frac{1}{2}$.

The rain at Paris in the month of July alone amounted to 3 inches 1 line $\frac{3}{8}$; which caused several rivers to overflow, and swept away the greatest part of the forage on the borders.

borders. On the 5th of July at Utrecht there was a prodigious rain; for from 11 o'clock in the morning to midnight there fell 47 lines $\frac{1}{2}$ of water, altho' there was a considerable intermission from the beginning till two o'clock in the afternoon. The wind was during that month to the south, inclining sometimes towards the east and sometimes towards the west. The rain in the month of December was likewise very abundant, it was 2 inches 9 lines $\frac{5}{6}$. There were towards the end of this month very violent winds from south-west; and on the 25th there was a great storm with thunder and lightning.

The winter was very mild, the greatest cold caused the liquor of the thermometer to descend only to 23 degrees $\frac{1}{2}$ on the 23d and 24th of January, with cloudy weather, and a high wind from north-east: On the 30th of November following it fell to 22 degrees $\frac{1}{2}$.

The greatest cold at Utrecht was on the 22d of January in the evening.

The heat of the summer was not great; the highest that the thermometer rose at Paris was on the 6th of September at three in the afternoon to 76 degrees, with a west wind.

At Utrecht the greatest heat was on the 19th of May, and on the 9th of July.

The barometer marked the greatest elevation of the mercury at Paris, on the 9th of February at 28 inches 6 lines, on the 16th of the same month at 28 inches 6 lines $\frac{1}{2}$, with cloudy weather, and a north-east wind. The mercury kept to a great height during the months of January and February.

The greatest height of the barometer at Utrecht was on the 22d of January, at 28 inches 10 lines $\frac{1}{2}$. Mr. Musschembroek had never seen the mercury rise so high. The wind was to the north.

The lowest state of the barometer at Paris was on the 26th and 27th of December, at 26 inches 11 lines, with cloudy and rainy weather, and a very high wind from south-west.

It was the 25th of December at noon that the barometer was the lowest at Utrecht at 26 inches 8 lines $\frac{1}{4}$; that day was memorable for a violent storm which occasioned several vessels to be lost on the coasts of Holland and England.

The 13th of May I observed with a needle of 4 inches the declination to be 15 degrees 35 minutes north-west: Mr. Buache observed it on the 1st of December with a needle of 6 inches, at 15 degrees 40 minutes.

Mr. Musschembroek makes with particular care the observations of the inclination, and declination of the needle; and he hath
found

found that there were sometimes between one day and another very considerable changes: The needles he makes use of are very large. The declination hath varied in the course of this year from 12 degrees 55 minutes, to 14 degrees; and in general the greatest declinations have been in the months of July, August, and September; but this rule is not without exception.

Anno
1735.

13 inches 10 lines $\frac{1}{2}$.

At Utrecht 26 inches 1 line $\frac{1}{8}$; and the evaporation 23 inches 2 lines Rhinland measure.

According to observations made at Rome this year by Mr. de Revillas, and sent to the Academy by Mr. Amelot, minister and secretary of state; the quantity of rain that fell in that city was 31 inches French measure, 0, 138.

The cold of this year was not considerable, for the thermometer fell no lower than to 27 degrees on the 5th of February, with a north-east wind; and on the 23d of December following to 26 degrees $\frac{1}{2}$, with serene weather, and a north-east wind.

The thermometer was lowest at Utrecht on the 26th of December.

The heat of the year was likewise very moderate; the liquor of the thermometer rose but once to 72 degrees $\frac{1}{4}$, on the 15th of July at two of the clock in the afternoon, with a south-east wind.

At

At Utrecht the greatest height of the thermometer was on the 16th of July at noon.

The barometer at Paris shewed the greatest elevation of the mercury on the 14th of February at 28 inches 6 lines, with cloudy weather and a north wind. It was several days before and after at 28 inches 5 lines.

On the same day at Utrecht the mercury was at 28 inches 7 lines $\frac{1}{2}$.

The least elevation of the mercury at Paris was on the 20th of January at 27 inches, with rainy weather, and a moderate west wind.

The least height of the mercury at Utrecht was on the 19th of January in the evening at 26 inches 5 lines $\frac{1}{4}$.

On that day there was a very memorable tempest in Holland, Germany, England, and at some of the ports of France, particularly at Brest; the fury of the storm was at various hours in different places, it was in its greatest violence a little after half an hour past 3 in the afternoon at Leyden; at Magdeburgh it was at half an hour past 4; and between 6 and 7 of the clock at Utrecht. During the storm the height of the barometer was less at Leyden than at Utrecht.

A table of the various heights of the mercury during the storm at Utrecht and Leyden.

At Utrecht.

At noon the barometer							inches	lines
was	-	-	-	-	-	-	at 27	11
At 3 o'clock	-	-	-	-	-	-	27	7
4 $\frac{1}{2}$	-	-	-	-	-	-	27	6
5 $\frac{1}{2}$	-	-	-	-	-	-	27	5 $\frac{3}{4}$
6	-	-	-	-	-	-	27	5 $\frac{1}{2}$
7	-	-	-	-	-	-	27	5
8	-	-	-	-	-	-	27	5 $\frac{1}{8}$
9	-	-	-	-	-	-	27	5 $\frac{1}{2}$
10	-	-	-	-	-	-	27	6
11	-	-	-	-	-	-	27	7

At Leyden.

At 2 o'clock in the afternoon the mercury

							inches	lines
was	-	-	-	-	-	-	at 27	6
At 3 o'clock	-	-	-	-	-	-	27	5
3 $\frac{1}{4}$	-	-	-	-	-	-	27	4 $\frac{3}{4}$
3 $\frac{1}{2}$	-	-	-	-	-	-	27	4 $\frac{2}{3}$
4	-	-	-	-	-	-	27	4
4 $\frac{3}{4}$	-	-	-	-	-	-	27	3 $\frac{1}{2}$
5 $\frac{1}{2}$	-	-	-	-	-	-	27	3
6 $\frac{1}{2}$	-	-	-	-	-	-	27	2 $\frac{4}{5}$
8	-	-	-	-	-	-	27	3
10 $\frac{1}{2}$	-	-	-	-	-	-	27	5 $\frac{1}{2}$

Note, All these measures are taken on the Rhinland foot, which is to that of France as 139 to 144.

Mr. de Mairan had observed the height of the barometer several hours of that day, and all

all reductions made, the mercury is found to have been lower at Paris than at Utrecht.

It must be observed, that the difference between the height of the mercury at Utrecht and at Leyden, is not quite so great as it appears by the tables; because the measures in those two towns are somewhat different.

On the 28th of April at Paris a needle of 4 inches declined 15 degrees 45 minutes towards the north-west. And on the 1st of October another needle of 4 inches declined 14 degrees 55 minutes.

The least declination of the needle at Utrecht was to 12 degrees 15 minutes on the 4th of November; and the greatest to 15 degrees 20 minutes on the 22d of June; it remained near a month beyond 15 degrees, and diminished afterwards.

Anno
1736.

15 inches 0 line $\frac{1}{3}$.

At Utrecht 22 inches 9 lines $\frac{2}{3}$; the quantity of evaporation was 28 inches 11 lines, greater than that of the rain, as it very often happens.

The cold of this year was very moderate, the thermometer having fallen only to 25 degrees $\frac{1}{2}$ on the 3d of January, and to 24 degrees on the 24th and 25th of February, with a north-east wind.

At Utrecht the greatest cold was on the 19th of February at 7 o'clock, in the morning.

The greatest heat at Paris was on the 30th of July, and on the 14th of August at 2 o'clock in the afternoon; for the liquor of the thermometer, which at sun-rising was at 68 degrees, rose in the afternoon to $80\frac{1}{2}$, the wind having turned to the south.

The greatest heat at Utrecht was on the 24th of July at 2 o'clock in the afternoon. There was that day at Utrecht a storm of hail, the stones of which were as big as pigeons eggs, and some as hens eggs; there were some 2 ounces in weight, and some sunk into the ground 2 inches deep. The needle which declined that day 13 degrees 25 minutes, declined after the storm 13 degrees 35 minutes.

The barometer at Paris shewed the greatest elevation of the mercury at 28 inches 5 lines $\frac{1}{2}$ on the 30th of November, the 1st, and 2d of December, with cloudy weather and a north-west wind: The least altitude was 27 inches 2 lines $\frac{1}{2}$ on the 26th and 28th of January, with a south wind and cloudy weather.

The greatest height of the barometer at Utrecht was 28 inches 3 lines on the 14th and 15th of November; and the least height 26 inches 7 lines on the 20th of October at 11 at night.

The declination of the needle at Paris in the month of December 15 degrees 40 minutes.

At

At Utrecht the greatest declination of the needle was 14 degrees from the 17th to the 25th of February, and on some other days of the year; the least declination was 12 degrees 15 minutes on the 17th of December at noon.

Anno
1737.

15 inches 10 lines $\frac{1}{6}$.

It is very surprising, that the rain which fell in two days alone of the month of June, namely, the 10th and 11th, exceeded in quantity that of the four first months of the year; for that of the 10th amounted to 10 lines, and that of the 11th to 1 inch 5 lines $\frac{2}{3}$, which makes 2 inches 3 lines $\frac{2}{3}$; whereas the rain which fell in the four first months, amounted only to 2 inches 2 lines $\frac{1}{6}$.

The barometer kept during the month of June at a great height, it fell only to 27 inches 8 lines $\frac{1}{2}$ on the 25th of that month at 3 o'clock in the afternoon, with a south-west wind and rainy weather; on the 10th and 11th of that month, the days of the great rain, it was at 28 inches, and the rest of the month it was oftner above 28 inches than below.

The cold of the year was very moderate; the greatest cold was on the 29th of December, when the old thermometer fell to 25 degrees: During the whole month of January, the liquor scarce descended to the freezing point.

The

The greatest heat of the summer was on the 16th, 17th, and 21st of July, when the liquor of the old thermometer rose to 75 degrees $\frac{1}{2}$ at 3 o'clock in the afternoon.

By observations made at Montpellier the greatest cold there was on the 5th of February at 7 o'clock in the morning, when Mr. de Reaumur's thermometer was 3 degrees below freezing; as it was at Paris on the 29th of December, which was the day of the greatest cold at Paris. The greatest heat at Montpellier was on the 21st of July, which was one of the days of the greatest heat at Paris; but Mr. de Reaumur's thermometer at Montpellier on that day was at 26 degrees $\frac{1}{2}$, and only at 25 degrees $\frac{1}{2}$ at Paris.

The barometer marked the greatest elevation of the mercury on the 30th and 31st of January at 28 inches 7 lines; and the least altitude at 27 inches 5 lines on the 15th of March.

I observed several times in the course of the year the declination of the needle at 14 degrees 45 minutes.

14 inches 9 lines.

Anno

The rain of this year hath been distributed through the several months in a manner very different from what is usually observed: For in the month of May, that is considered as one of the most serene in the year, we measured near 3 inches 8 lines of water, which equals pretty nearly the quantity observed through

through June, July, and August, which are the months wherein commonly falls the most rain, occasioned by the storms that happen at that season.

It is perhaps this *unseasonableness* of the rains, that was in part the cause of the crops of grain not being plentiful in several provinces of France.

The greatest cold was on the 8th of January in the morning, the weather serene, and the wind north; the thermometer fell to 21 degrees $\frac{1}{4}$.

The greatest heat was on the 5th of August, and it was felt the same day in divers provinces of France as one of the greatest that had been ever known. The old thermometer rose to 82 degrees $\frac{1}{4}$. On the 30th of January, the 5th and 17th of February the barometer was at 28 inches 6 lines $\frac{1}{2}$.

The barometer was the lowest on the 22d of March, the mercury being at 27 inches 1 line $\frac{1}{2}$.

On the 28th of March the declination of the needle was 15 degrees 10 minutes.

19 inches 1 line $\frac{1}{8}$.

Anno
1739.

This was the most rainy year that we have had ever since the year 1713; yet it was only as a common year of rain with regard to the years preceding that time.

The greatest cold was in the month of November from the 24th to the 28th, with serene weather and a gentle east-wind: The
old

old thermometer was fallen on the 24th to 27 degrees $\frac{1}{2}$, on the 25th to 26 degrees $\frac{1}{2}$, on the 26th to 15 degrees, on the 27th to 23 degrees. The thermometer is always kept in the same place.

The greatest heat of the summer was on the 20th, 21st, and 22d of July; on the 20th the thermometer rose in the afternoon to 68 degrees $\frac{1}{2}$, on the 21st to 73 degrees, and the 22d to 75 degrees $\frac{2}{3}$.

The barometer marked the greatest height of the mercury at 28 inches 3 lines $\frac{1}{2}$, on the 5th, 6th, and 7th of March, with a north-east wind; and the least height at 26 inches 8 lines $\frac{1}{2}$, on the 5th of February, with a south wind.

There were during the months of January and February very high winds from south-west, and particularly the night between the 15th and 16th of January, which was accompanied with a great storm together with thunder and lightning, that lasted a long time: The wind in the night between the 17th and 18th was still more violent than the former.

I observed several times in the month of December the declination of the needle 15 degrees 30 minutes.

21 inches 6 lines $\frac{2}{3}$.

Anno

The month of December alone, in which there fell 5 inches 0 line $\frac{2}{3}$, gave almost as much as the first six months of the year;

1740.

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which

which occasioned a considerable over-flowing of the river, that was greater than in the month of February 1711, but less than that which happened in 1658; but I have been told, that the great inundation in 1658 at Paris was increased by the fall of a bridge, which of a sudden barred the passage of the river.

The cold in the beginning of the year was very great. The liquor of the old thermometer, the same that Mr. De la Hire made use of in 1709, fell to 14 degrees on the 10th of January, with cloudy weather, and a slack north-east wind. It rose to 15 degrees $\frac{1}{2}$ the 11th, and to 27 degrees $\frac{1}{2}$ the 12th of the same month. The liquor of the same thermometer fell to 5 degrees on the 13th and 14th of January in 1709, which shews, that the cold was *then* much greater than in 1740. The cold continued this year a long time. The heat of the summer was not great; the thermometer rose but once to 69 degrees $\frac{1}{2}$, which happened on the 15th of July in the afternoon; in the morning it was at 58 degrees. The greatest height of the barometer was 28 inches 5 lines $\frac{1}{2}$ the 30th of June, and the 1st of July, with a gentle wind from north-north-east. On the 10th and 11th of January, the days of the greatest cold, the mercury was at 27 inches 3 lines. On the 4th of December following it fell to 26 inches 11 lines at 7 o'clock in the

the evening, with a great storm, and a high wind from south-west, which lasted a great part of that month.

By the observations of father Perot, Jesuit, professor of mathematics at Lions, the greatest cold *there* was on the 19th of February. The greatest heat at Lions was on the 19th of July.

We observed several times in the summer the declination of the needle 15 degrees 45 minutes.

Mr. de Reaumur observes, that the winter of 1740 was chiefly remarkable for its long continuance. The birds that quit *us* before winter to visit *us* again in the spring,

Observations on the frost of 1740.

—*Te, dulcis amice, reviset*

Cum Zephyris, si concedes, et hirundine primâ,

plainly shewed that they are not informed of the actual state of our climate when they *remove* to it; they probably leave the countries whither they *retire to*, when they cease to find their food. The swallows particularly were unhappy in not knowing what had passed here with us; the greatest part of those that came the first, came only to die by hunger: They feed on little flies and gnats which they catch in the air; and in the year 1740 the air was peopled more late than usual with those insect nations, of which the greatest number pass the winter under the form of nymph or

worm : We have proved elsewhere that heat advances the transformation of these insects, and that cold retards it. The swallows therefore fatigued with ranging the fields of air, which did not afford the game that was necessary for their livelihood, fell to the ground without strength, and perished for want of nourishment. Mr. Bazin hath sent me an account from Strasbourg, that the swallows having arrived as usual in Alsace the beginning of April, and not finding insects, fell every hour of the day at the feet of passengers in the streets. I have taken up several in my hands in my gardens at Charenton, that were unable to fly ; and they were picked up dying or dead in many places of Paris and the neighbourhood.

I know no kind of insects to which the cold of this winter occasioned any great mortality but the vinefretters, those destroyers of the honeysuckle ; in consequence of which those who delight in gardens have had the pleasure of seeing their honeysuckles in beauty the whole summer.

Several experiments and observations prove that those insects, to whom it hath been *prescribed* to live exposed in the winter to all the severity of cold, are in a condition to bear a much greater cold than that of 1709 ; and that those whom that degree of cold, or even a less degree would cause to perish, have the art to hide themselves under ground to a
depth

depth where the earth cannot receive that degree of cold which is necessary for freezing: They have not need for that purpose of going so deep as one might imagine, for the cold hath more difficulty to penetrate within the earth, when the first crust is frozen, than is commonly thought. After frosts much sharper than those of the year 1740, and which approached to that of 1709, and lasted longer than eight days, I caused a plat of ground to be dug, which in autumn had produced pease, and consequently the earth must be loose; I found that the frost had not penetrated to the depth of 4 inches: Thus insects, that can *work* themselves into earth, run no risk of being exposed to too violent a cold.

By the experiments of Captain Middleton, at Hudson's-Bay, the frost penetrated ten or twelve feet under ground. Vid. Martyn's abridgement of the Philosophical Transactions, part 11, page 471.

The consequences of this winter were very proper to undeceive those who imagined that in years, when the winter hath been mild, there reign more diseases than in those which have had a severe winter, and who think the continuance of the cold necessary to purify the air; for the year 1740 may be ranked in the number of those which have been remarkable for the mortality in the spring

X 3

throughout

throughout this kingdom. In the greatest part of its provinces the country hath lost a prodigious number of inhabitants; I know villages in Poictou from which half the inhabitants have been *taken off*.

This year may serve likewise to nip an ancient prejudice, that a very cold winter is succeeded by a hot summer.

The abbé Nollet hath received an account from London, that the greatest cold *there* was on the 5th of January in the morning, and was expressed by a little more than ten degrees of our (*Mr. de Reaumur's*) thermometer. According to these observations the cold was felt sooner at London than at Paris, but was not greater.

The days on which the cold was the greatest at Paris were the 10th of January, and the 25th of February; in the first the liquor of *Mr. de Reaumur's* thermometer descended to 10 degrees $\frac{1}{4}$ below freezing, and in the latter to 10 degrees $\frac{1}{8}$.

We do not know exactly what was the degree of cold in 1709, marked by *Mr. de Reaumur's* thermometer; but we are very well satisfied that it did not exceed 15 degrees $\frac{1}{2}$ below freezing.

This year on mount Cenis, where the abbé Nollet in his return from Turin to France left a thermometer of *Mr. de Reaumur* with *Mr. Gressy*, priest and rector of the hospital established on that high mountain,

tain, the liquor of the thermometer in the month of February descended to 16 degrees below freezing ; which marks a greater cold than what we had at Paris in 1709. Mr. Gressy hath written to the abbé Nollet a letter bearing date the 15th of October 1740, in which he mentions that on the 12th, 13th, and 14th of that month, the liquor was already fallen to 9 degrees below freezing.

Mr. Muffchenbroëk observes, that in Holland there had not been in the memory of man any cold equal to that of this year : On the 11th of January it surpassed at *Leyden* what we felt at Paris in 1709, for the liquor of Mr. de Reaumur's thermometer descended *there* on that day to 16 degrees $\frac{1}{2}$ below freezing. Its other effects were proportionate to what it produced on the thermometer. Mr. Muffchenbroëk saw in February ice which was 26 inches in thickness, and there had been still *thicker* in January. (Vid. Vol. 1st, Page 5th.)

The greatest cold in January was one day later at *Leyden* than at Paris, and the greatest cold in February was on the *same* day, *namely*, the 25th, at both towns.

We think, and we have good reason for thinking, that the great frosts come to us hither from the north ; it must, therefore, appear singular that the most violent cold at Paris in January 1740, should be felt on the

10th of that month, and that the greatest cold at Upsal in Sweden should not be perceived till the 25th of the same month, 15 days later. All that we should conclude from it however is, that the winds which had brought to Upsal the cold of the 25th, had not reached to us, or that they were crossed in their passage by other currents of air which warmed them; for in the days succeeding the 25th, the degree of cold was not very sharp at Paris: But it was very considerable at Upsal on the 25th, since it caused the thermometer of Mr. de Reaumur to descend to 19 degrees below freezing.

Mr. de Maupertuis observed at Torne, in the month of January 1737, the thermometer of Mr. de Reaumur at 37 degrees below freezing. Vid. Vol. 1st, Page 280.

N. B. Mr. du Hamel began this year a very useful work, which he intitled Botanico-Meteorological Observations; from which I shall give particular extracts in the botanical part of this work.

Anno
1741.

12 inches 10 lines.

The rain of the month of April, which contributes much to the crops of hay, was only 2 lines; in consequence of which the burden was very small, but it was a little repaired by the *after-grass* in autumn, which came after the rains in September.

The

The cold of the winter was not great; the liquor of the old thermometer was fallen on the 26th of January, in serene and calm weather, to 18 degrees. During the months of February, March, and April, it scarce reached to freezing.

The greatest heat of the summer was on the 7th and 8th of August; on the 7th of August about sun-rising the thermometer was at 61 degrees $\frac{1}{2}$, and rose in the afternoon to 73 $\frac{1}{2}$; on the 8th of August in the afternoon it was at 75 degrees $\frac{1}{2}$.

The mercury of the barometer kept at a great height the whole year; it was at 28 inches 7 lines, the 18th of February, and at 28 inches 6 lines $\frac{1}{2}$ the 23d of November, with a great fog. It was several times at 28 inches 6 lines, with different sorts of weather, sometimes clear and sometimes foggy. Its least height was at 27 inches 5 lines $\frac{3}{4}$, on the 19th of September, and at 27 inches 6 lines $\frac{1}{2}$ on the 20th of the same month, with rainy weather.

I observed several times during the year the declination of the compass with a needle of 12 inches, and found it 15 degrees 35, or 40 minutes north-west.

The quantity of rain that fell at Leyden this year, from the observations of Mr. Muffchenbroëk, was 18 inches, which is *there* reckoned a dry year, the quantity being *communibus annis* 24 inches; it was one of the
most

most remarkable years ever known in Holland for the abundance of their harvest, and the fineness of the weather.

The greatest height of the mercury in the barometer at Leyden this year was on the 17th of March, with cloudy weather, and a gentle north-west wind, at 28 inches 7 lines $\frac{2}{3}$; the least height was 27 inches 4 lines $\frac{1}{10}$, with cloudy weather, and a very high wind from west.

On the 7th of August at 20 minutes past 10 o'clock at night, there was seen at Leyden to rise from the earth, and mount into the air, a fire resembling a flash of lightning, which diffused so great a light that one might have seen upon the floor a very fine needle. This light was very brilliant, and accompanied with a little noise; it lasted no longer than a flash of lightning, and formed in the sky a spout of fire about 15 minutes broad, which extended in meanders the length of 20 degrees. It did not emit any ray; by little and little it assumed the figure of a globe, and was changed into a white and luminous cloud, so thick that it concealed the stars; but by degrees it became less thick, and resembled the *via lactea*, through which you perceived the stars: This cloud began to disappear at the east, and intirely vanished at the west.

N. B. *There have been many luminous meteors mentioned in the course of the history of the*

the Academy, which I have not troubled the reader with; because they were of the same kind as that mentioned vol. 1st, page 37: But this being somewhat singular, and coming from so good an authority as Mr. Musschenbroëk, I was unwilling to omit it. I beg leave to observe, that any uncommon luminous meteors were sometimes called by the ancients cometæ; thus Virgil,

Non alias cœlo ceciderunt plura sereno
Fulgura: nec diri toties arserunt cometæ.

The quantity of rain that fell at Upsal this year, observed by Mr. Celsius, was 16 inches 9 lines $\frac{2}{3}$.

The greatest height of the barometer there was 28 inches 8 lines $\frac{1}{4}$, on the 15th of March, with serene weather, and a moderate north wind; and on the 16th of October, with cloudy weather, and the wind W. N. W.

The least height was 27 inches 2 lines, on the 9th of September, with rainy weather, and a north-west wind.

At Montpellier, according to the observations of Mr. Bon, the cold was greater in 1741, than in 1740; for in the latter the liquor of Mr. de Reaumur's thermometer did not descend to freezing; whereas in 1741 it fell to 3 degrees below freezing.

12 inches 9 lines $\frac{1}{2}$.

Anno
1742.

Which shews it to have been a dry year; yet the harvest and vintage were plentiful, but

but the crop of hay was very small. The husbandmen have attributed the abundance of corn to the long continuance that the snow, which fell the 2d of January and the 1st and 2d of March, made upon the ground and the smallness of the crop of hay to the dryness of the month of May.

The cold of this year was very great, and the greatest that had been felt ever since the year 1709. It began with snow, which fell the 2d of January, and continued increasing till the 10th. The old thermometer on that day was at 8 degrees $\frac{7}{8}$. The severity of the winter continued a long time.

The heat in the beginning of July was very considerable, it approached nearly to the greatest heat that hath been observed in this country. The liquor of the old thermometer rose on the 2d of that month at half an hour past 2 in the afternoon to 80 degrees.

The mercury in the barometer kept at a great height during almost the whole year; it was at 28 inches 6 lines on the 13th of April, with serene weather and a high wind from north-east; at 28 inches 5 lines $\frac{1}{2}$, on the 11th and 12th of the same month, with cloudy weather, and a gentle north east wind and on the 15th, with calm and serene weather.

The least height was 27 inches 2 lines on the 11th of October and the 4th of December.

ember, with cloudy weather and a gentle south east-wind.

On the 26th of May and the 2d of June, a needle of 12 inches declined 15 degrees 40 minutes; on the 21st of September a needle of 4 inches declined 15 degrees 10 minutes.

The depth of rain that fell this year at Leyden was 23 inches 6 lines $\frac{1}{4}$; but the first six months were very dry, there having fallen only 5 inches 6 lines.

The greatest height of the mercury in the barometer was 28 inches 6 lines $\frac{1}{3}$ on the 12th of May, with serene weather and a north wind.

The least height was 26 inches 9 lines $\frac{4}{5}$ on the 14th of October at noon, with a high wind from south-west and cloudy weather; the mercury was in the morning at 27 inches 10 lines.

Mr. Muffichenbroëk reports, that there hath been a great quantity of field-mice, which have ravaged the meadows and the fields; they have intirely eaten up the grass of some pasturages, and when grass failed, they destroyed the leaves and fruits of trees: the number of them was so great, that a single countryman hath killed five or six thousand in a day. He mentions, that he had seen the borders of some ditches pierced like a sieve with holes, where they lodged, and had counted fifty of these holes in the space of a square perch.

The

The great cold that there was this year in the neighbourhood of Paris, and in the greatest part of France, was not perceived in Languedoc; on the contrary, at Montpellier it hath been less than in the preceding year: the liquor of Mr. de Reaumur's thermometer fell no lower than 1 degree below freezing on the 4th and 5th of January and the 22d of February; whereas in 1741 it fell to 3 degrees below freezing. The heat likewise at Montpellier this year hath been less than in 1741, for the liquor of the thermometer rose no higher than to 26 degrees on the 5th of August; whereas in 1741 it rose to 28 degrees on the 24th of July, and the 12th of August.

Anno
1743.

13 inches 2 lines $\frac{1}{3}$.

The greatest height of the barometer was 28 inches 7 lines on the 2d and 21st of January, the 12th and 23d of December following, with great fogs; and the least height was 27 inches 6 lines $\frac{1}{4}$ on the 18th of July with cloudy weather, and a high wind from south-west.

Mr. Allaman of Leyden hath sent word to the abbé Nollet, that on the 2d of January this year, with a west wind, and during an abundance of rain, the barometer was higher at Leyden than had been known for 40 years; the mercury was mounted to 28 inches 8 lines $\frac{2}{3}$. The height at Paris that day was greater considering the different elevation

tions of the two places. By divers levellings of the river Seine from Paris to its mouth it appears, that the great hall of the observatory is between 45 and 46 fathom higher than the level of the sea, which answers to about 4 lines $\frac{1}{2}$ of height of mercury, and would give to the barometer on the sea shore that day 28 inches 11 lines $\frac{1}{2}$; now *Leyden* in its situation being much less high than Paris, the height of the mercury *there* was not so great in proportion as with us.

The greatest heat of this year happened on the 17th of June, when the liquor of the old thermometer rose at 3 o'clock in the afternoon to 74 degrees $\frac{2}{3}$: The greatest cold was on the 7th of January, when the liquor of the old thermometer fell to 22 degrees.

I observed several times in the course of the year, with a needle of 4 inches, the declination to be 15 degrees 10 minutes.

16 inches 10 lines.

Anno
1744.

The cold of this year was pretty sharp; the Seine was frozen on the 11th of January in the morning with a north-east wind gentle and serene; the old thermometer was then at 21 degrees $\frac{1}{2}$. The 14th of January was the coldest day of the year; the thermometer was at 20 degrees.

The me-
teorologi-
cal obser-
vations
continued
by Mr.
De Fou-
chy.

The greatest heat was on the 29th of June in the afternoon; the old thermometer was at 73 degrees, the wind north-west and gentle, and the sky serene.

The

The mercury kept at a pretty great height during this year ; it was at 28 inches 7 lines the 4th of January, with a slack north-east wind and serene weather ; and the 26th and 28th of the same month, with a great fog. The least height of the barometer was 27 inches 5 lines the 14th of April, with a high wind from south-west.

On the 17th, 18th, 20th, and 21st of July a needle of 4 inches declined 16 degrees 15 minutes.

This year the disorder amongst the horned cattle committed great ravages in Burgundy. By curious experiments made by the marquis de Courtivron it appeared, that the hides taken from cattle that had died with the infection, were not contagious to the species, and consequently that they might with safety be dressed into leather.

Of the
cotbing of
sheep.

I beg leave to take this opportunity of mentioning another pestilential disorder that is common amongst sheep, and is called in the western parts of England cotbing : The origin of this disorder was discovered by the famous Mr. Pecquet, the great discoverer of the thoracic duct.

Extract of a letter from Mr. Pecquet, July 9, 1668.

“ Last monday we were assembled in the
“ king’s library, in order to search in several
“ animals for a confirmation of a duct which

" we had found in *some* to convey the bile
 " into the cystis, and the mouth of which is
 " closed by a valve of a very particular struc-
 " ture, that hath not yet been described.
 " As we were searching for this valve in the
 " liver of a sheep, we found in the cystic
 " duct amongst the bile several worms that
 " were still alive.— Our butchers assure us,
 " that the worms which we had found are
 " very often seen in the livers of sheep, but
 " that in truth it is only in such as are dis-
 " eased; and that they have observed, that
 " this happens to the sheep when they have
 " eaten of a certain herb, which we found
 " to be the *fideritis glabra arvensis*.

" But the remark that the butchers make
 " upon it is very curious, which is that these
 " worms are intirely resembling the leaf of
 " the herb; which we found in effect to be
 " true." *Vid. Tom. 10, pag. 476.*

12 inches 5 lines $\frac{4}{8}$.

Ann^o

At Nismes the quantity of rain that fell ^{1745.}
 this year was 43 inches 3 lines, insomuch
 that the whole country hath been almost
 drowned with floods.

The quantity of rain at Toulon was 27
 inches 5 lines $\frac{7}{8}$; which is much beyond what
 commonly falls even in rainy years.

The greatest cold was on the 14th of Ja-
 nuary; the old thermometer marked 11 de-
 grees $\frac{1}{2}$.

The greatest heat was on the 6th of July; the thermometer was at 69 degrees.

The greatest cold at Nismes was on the 20th of January, the thermometer of Mr. de Reaumur marking 8 degrees: At Paris on the 14th of January, which was *there* the coldest day, the thermometer of Mr. de Reaumur marked 10 degrees $\frac{1}{4}$. At Nismes the hottest day was the 17th of July, the same thermometer marking 31 degrees $\frac{1}{4}$. On the 6th of July, which was the hottest day at Paris, the thermometer of Mr. de Reaumur was risen to 24 degrees $\frac{1}{2}$ above freezing.

The barometer at Paris shewed the greatest elevation of the mercury at 28 inches 9 lines * on the 20th of February, with a cold east wind: It fell the lowest to 27 inches 4 lines on the 26th of November, with a south-west wind and a great fog.

At Toulon the barometer rose the highest on the 3d of January, the mercury being at 28 inches 5 lines with calm and fine weather: The least height of the mercury was 27 inches 4 lines on the 1st of December, the wind at west-north-west.

On the 17th, 18th, and 19th of May, a needle of 4 inches declined at Paris 16 degrees 15 minutes towards the north-west.

A

* *This is the highest that hath been mentioned through the whole course of the register.*

A needle of 4 inches at Toulon declined during the first six months of the year near 17 degrees.

The thermometer of Mr. de Reaumur fell the lowest at Toulon on the 23d and 24th of January, marking 7 degrees $\frac{1}{2}$; and it rose the highest on the 16th of July, marking 24 degrees.

Mr. Sarrau hath written from Bourdeaux that there had fallen in that town this year 30 inches 8 lines of water.

By comparing all these observations, it appears, that there hath fallen much more water towards the southern parts of France than at Paris, and that the greatest *cold* and the greatest *heat* which happened at Paris, have preceded by some days *those* of Nîmes and Toulon.

14 inches 5 lines $\frac{1}{6}$.

Anno
1746.

The greatest cold was on the 15th of February; the liquor of the old thermometer fell to 18 degrees $\frac{2}{3}$. That of Mr. de Reaumur marked 7 degrees $\frac{1}{4}$.

The greatest heat was on the 15th of July: the liquor of the old thermometer rose to 69 degrees $\frac{3}{4}$. That of Mr. de Reaumur marked 26 degrees $\frac{1}{4}$.

The greatest elevation of the mercury was to 28 inches 5 lines on the 22d of November, with a violent north-west wind. It fell the lowest to 26 inches 11 lines on the 13th of
Y 2 March,

March, with a south-west wind accompanied by a fog.

On the 24th and 25th of June, a needle of 4 inches declined 16 degrees 15 minutes north-west.

This year Mr. Malouin began a very useful work, containing monthly observations on the effect of the variations of the air in different diseases; of which I shall give an account in the medicinal part of this work.

The number of births this year at Paris was of legitimate 18347, of illegitimate 3282, total 21629: Of which 11040 males, and 10589 females. The number of deaths amounts to 18051, namely, of males 9418, of females 8633.

Anno
1747.

16 inches 9 lines $\frac{2}{6}$.

The greatest cold of the year was in the night between the 13th and 14th of January: The thermometer of Mr. de Reaumur marked 11 degrees $\frac{3}{4}$ below freezing, and the old thermometer was fallen to 11 degrees.

The reader will please to remember that the freezing point of the old thermometer was 32 degrees.

The greatest heat was on the 6th of September: the thermometer of Mr. de Reaumur was at 27 degrees $\frac{1}{2}$ above freezing; and the old thermometer was at 77 $\frac{1}{2}$.

Th

The greatest height of the barometer was 28 inches 1 line on the 9th and 10th of March, with a north-east wind: It fell the lowest to 26 inches 11 lines on the 23d of November, with a very violent wind from S. S. W.

On the 20th, 21st, and 22d of July, a needle of 4 inches declined 16 degrees 30 minutes north-west.

The Academy hath already related several instances of the danger of sounding bells, or of exciting any other violent commotion in the air, when a storm is hovering over you. Mr. du Hamel hath mentioned a new accident of this kind, which happened near Pluviers; the thunder fell precisely on the steeple where they sounded the bells. It is to be wished, that reason and experience could destroy a prejudice so contrary to all the laws of physics, and whose consequences might prove so fatal.

The number of births this year at Paris was 21815, *namely*, legitimates 18446, illegitimates 3369. The number of deaths 18158, of which 9592 males, and 8566 females.

There were admitted into the Hôtel-dieu, in the course of this year, 17958 patients.

17 inches 3 lines $\frac{5}{8}$.

Anno

The greatest cold was in the night between the 12th and 13th of January: The liquor of the old thermometer was fallen to 12 degrees

grees $\frac{1}{2}$; and that of Mr. de Reaumur to 10 degrees $\frac{1}{8}$ below freezing.

The greatest heat was on the 23d of June; the old thermometer rose to 82 degrees $\frac{1}{4}$; and Mr. de Reaumur's to 29 degrees $\frac{1}{2}$ above freezing.

On the 26th of November, with a south-east wind and a great fog, the mercury was risen to 28 inches 6 lines $\frac{1}{2}$; and on the 26th of December, with a very violent south-west, it was fallen to 26 inches 5 lines.

There were born in the course of this year 21336, of whom 17907 legitimate, 3429 illegitimate, 10959 males, and 10377 females.

There died 19529, of whom 10265 were males, and 9264 females.

Note, That we always include the deaths in religious houses, of foreigners, &c.

The number of patients received into the Hôtel-dieu in the course of this year was 19691.

Anno
1749.

19 inches 0 lines $\frac{1}{2}$.

The greatest cold was in the night between the 8th and 9th of February; the old thermometer was at 18 degrees, and Mr. de Reaumur's at 6 degrees $\frac{3}{4}$ below freezing.

The greatest heat was on the 13th of July at one o'clock in the afternoon: the liquor of the old thermometer rose to 83 degrees (*which it had never done before*) and Mr. de Reaumur's to the same height as the
last

last year, *namely*, to 29 degrees $\frac{1}{2}$ above freezing.

On the 29th of November, with an east wind, and a thick fog, the mercury rose to 28 inches 6 lines; and on the 18th of February, with a violent south-south-west wind, it fell to 26 inches 4 lines.

The 10th and 11th of June a needle of 4 inches declined 16 degrees 30 minutes north-west.

Advertisement. The quantity of rain that falls at a *medium* at Paris was at first established at about 19 inches, by comparing the ten first years; but in 1708 or 1709, that is to say 20 years after the beginning of these observations, it was necessary to reduce the *medium* to about 18 inches 8 lines. In 1718 complete, it was still nearly in the same proportion; but in 1728 it was reduced to 17 inches 3 lines; and lastly, in 1743 the *medium* deduced from the sum total in the space of 55 years, was no more than about 16 inches 8 lines. We receive this computation from a note that Mr. Maraldi hath communicated to us on this subject.

History 1743, pag. 16.

The total number of persons dead at Paris in the course of this year 1749, amounts to 18607.

There have been born in the same time 22933, of which number 3775 have been carried to the Foundling Hospital.

There were admitted into the Hôtel Dieu, in the course of the year, 21082 patients.

Various
instances
of exces-
sive cold-
ness.

Nothing perhaps would be more uncertain in physics than the degrees of cold and heat, if we were reduced to refer them to the testimony alone of our *feeling*: It was necessary therefore to contrive some method of *comparing* the degrees of heat and cold, by exact and precise measures; and this method is the thermometer. Before the invention of this instrument, there was no way of knowing the different degrees of cold but by the effects, and it is in this manner that some historians have been able to preserve to posterity the remembrance of some memorable winters. Calvisius reports, for example, that in the year 859 of the Christian æra, the Adriatic sea was frozen in such manner that one could go on foot from *Terra Firma* to Venice. The same thing happened, according to Sydenham, in 1709.

I do not know what Sydenham they mean; but it cannot be the famous Sydenham.

The degree of cold in 1709 hath been a long while considered as the greatest ever known in this climate; and indeed the sad consequences it had, and which have but too well preserved the memory of it, gave

reason

reason to think that a greater degree of cold would be capable of destroying all organized bodies in the climate where it should be felt: We were confirmed still more in this notion by the cold that had been observed in Iseland at the same time, which was found to have been less severe than what was felt at Paris.

But since observers have been multiplied, and the genius of science hath spread itself into the more northern parts of Europe, we find that the *degree* of cold, which had been considered as the *greatest* that organized bodies can sustain, fell very short of *that* which is experienced every year in certain climates, without any considerable injury to the men, animals, or plants of the country.

By the observations of Mr. Delisle, who resided several years at Petersburg, the cold of the 27th of January 1733, caused the thermometers, which were exposed to the open air, to fall to the degree that answers to the 27th below freezing in Mr. de Reaumur's. By considering that the cold of 1709, would have made the thermometer of Mr. De Reaumur descend only to 15 degrees $\frac{1}{2}$, we shall easily form a judgment of the rigour of the season at Petersburg: This was the first great frost that had been observed there with exactness; but altho' it may appear to us extreme, and that whilst it lasted no one could be exposed to the air, even wrapped in the best furs; yet Mr. Delisle
2 hath

hath heard since his return, that in the year 1747, and the beginning of 1748, they had observed a much more severe frost at Petersburg, the thermometer having fallen to the degree that answers to the 30th in *that* of Mr. de Reaumur.

How intense soever this last degree of cold may appear; yet it is still but moderate, if compared with what hath been observed in different parts of the world.

For example, the gentlemen of the Academy that went into Lapland, in order to measure the degree of the polar circle, found the thermometer of Mr. de Reaumur to fall to 37 degrees below freezing. Vid. vol. 1st, page 280.

Probably they must have felt a cold very nearly approaching to it at Quebec in 1744. Mr. Gautier reckons that his thermometer would have fallen to the 33d degree of Mr. de Reaumur's; we say *reckons*, for the mercury having been collected into the ball after the 32d degree, he could not have the utmost boundary of cold but by estimation. A violent coldness, altho' not equal to the former, was felt at Astracan in 1746, the thermometer *there* having fallen to 24 degrees $\frac{1}{2}$ below freezing.

It is worthy of remark that Quebec, and Astracan, are placed nearly under the parallels of 46 or 47 degrees, which answers to the heart of France; an evident proof that
the

the degree of cold doth not always depend on the latitude of the place, where it is observed. We shall still be better convinced of *this* by observing that at Kirenga, on the frontiers of China, the cold hath been found to make the thermometer of Mr. de Reaumur descend to 66 degrees $\frac{2}{3}$, altho' this town is in the *latitude* only of 57 degrees and half, which corresponds pretty nearly with *that* of Riga, and the north of Scotland, where they experience nothing like it.

The greatest cold which Mr. Delisle hath been able to discover is, that which was observed at Yeniseisk in Siberia, on the 16th of January 1735 in the morning, the thermometer having fallen for some hours to 70 degrees below freezing; we say *some hours*, because in reality two hours before, and two hours after, it was a great deal higher.

This last mentioned cold is the greatest that hath been hitherto observed.

There are some curious observations on the natural history of Siberia, and its excessive coldness, in the Flora Sibirica, five Historia Plantarum Sibiricæ; of which an extract hath been given by the ingenious Dr. Fothergill, in the Philosophical Transactions N^o. 486, Page 248.

If we may judge by the effects, we shall find perhaps as terrible instances of cold related in several voyages.

There

There is, for example, the utmost reason to believe that it was to an equal degree of cold that Captain Willoughby was exposed, when seeking the passage to China by the north sea in 1553, the ice stopped him at Arzina in Lapland under the latitude of 69 degrees, where he was found dead with all his crew the year following.

The Dutch who, in attempting the same passage, were obliged to spend the winter at *Nova Zembla* in 1596, preserved themselves from death, which the rigour of the season would infallibly have caused, merely by shutting themselves up in a close hut, and keeping a continual fire; notwithstanding this care they had a good deal of difficulty to keep their feet from being frozen; their clothes and their furs were continually covered with ice, and the sherry wine was so perfectly frozen into a mass, that they distributed it one to another by pieces.

But to form a judgment agreeable to the precautions that are usually taken against cold, and which Mr. Delisle, who hath taken them a long while, is better acquainted with than any one; we know nothing comparable to the cold that Captain Middleton experienced in the habitation of the English in Hudson's-Bay, under the latitude of 57 degrees 20 minutes.

Vid. Martyn's Abridgment of the Philosophical Transactions, Part II, Page 469.

The

The houses of this habitation are built of stone, and their walls are two foot thick; the windows are very narrow, and furnished with thick shutters on the inside, which are kept closed at least 18 hours every day. They make up great fires in stoves four times in the day, and carefully stop up the chimnies as soon as the wood is brightened into a glowing coal; they light themselves at night with red-hot * bullets twenty-four pounders, suspended before the windows. Notwithstanding all these precautions, their liquors, not even excepting the brandy, are frozen in the smallest and the warmest closets; and all the inside of the chambers, and the beds are covered with a crust of ice several inches thick, which they are obliged to scrape off every day.

Whatever furs they wrap themselves in, no one is able, during this rigorous cold, to expose himself to the external air without danger of losing, when he returns into warm places, the skin from off his face and hands, or even having his toes and fingers frozen. The lakes of stagnating waters, which are no more than 10 or 12 feet deep, freeze quite to the bottom: The sea freezes pretty near the same thickness; altho' the ice may be no more than 8 or 9 feet deep at the mouth of rivers, and in places where the tide is strong, those enormous masses of ice sometimes

* *In the French, Boulets de vingt-quatre, rougis au feu.*

times split with an horrible noise, which equals the sound of the loudest cannon.

*Æraque diffiliunt vulgo, vestesque rigescunt
Indutæ, cæduntque securibus humida vina,
Et totæ solidam in glaciem vertère lacunæ.*

As to the earth, Mr. Middleton thinks that it is never intirely unfrozen ; for having caused it to be dug to the depth of five or six feet during the two months of summer, he found it frozen and white as snow.

This agrees with the poetical, and, which hath hitherto been thought imaginary, description in Virgil,

*Sed jacet aggeribus niveis informis et alto
Terra gelu late, septemque adfurgit in ulnas.*

Semper hiems, semper spirantes frigora cauri.

However plausible therefore the reasons were for thinking, that a cold much greater than that of 1709, would not have let either animals or plants subsist ; yet experience hath decided the contrary : Physical assertions which are not immediately supported by facts,

*Are like the baseless fabric of a vision ;
And like this unsubstantial pageant faded
Leave not a wrack behind.*

History 1749, pag. 1.

One

One is apt to think that nothing is better known than dew; we give this name to a moist vapour that is found in the morning on the ground, and on the leaves of plants in the country,

Of dew,
by Mr.
du Fay.

*Frigida vix cælo noctis decesserat umbra,
Cum ros in tenera pecori gratissimus herba :*

We call a *dampness* that which appears to fall in the evening, and is perceived to wet linen and clothes: It is *that* in the morning only which is called *dew*. One might imagine from this distinction, that there is an *interruption* in this moisture, and that it is not equal through all the hours of the night; but observations shew the contrary, and inform us, that if there are in the night any intervals of time, wherein it is not perceived, there are likewise whole nights which are intirely exempt from it; and this is owing to particular causes, such as wind, weather, cold, &c.

Those who have made no other reflections on dew than such as present themselves naturally, think that all the moisture one finds in the morning on plants and upon the ground comes in reality from *above*; but when the affair is examined with more attention, one sees that at least a great part of the dew arises from the earth itself and the plants, and keeps hanging to their surface. Mr. Gersten who hath made a dissertation

fertation * on this subject in particular, hath even thought that there fell no dew from *above* on the ground, and that all which we see under the form of dew arose from the earth or plants, on which it keeps hanging in pearls till the heat of the day hath dispersed it.

This opinion of Mr. Gersten was not new, and we find in the history of the Academy for 1687, that some persons of the company had advanced, that the dew rose from the earth and did not descend from *above*, because they found under glass-bells as much dew as in other places exposed to the air; it is probable, that since that time several persons have been of the same sentiment; one finds even traces of it in ancient authors: But Mr. Gersten did not derive his notion from thence; he had given in the work which we have cited a new explication of the risings and fallings of the barometer that did not agree with the *descent* of dew; this made him think of examining the thing with more attention, than had hitherto been done. He had already observed in gardens that the grass was full of a moisture in the evening, and covered with very perceptible little drops, whilst the leaves of trees, and plants that were more raised from the ground, had not the least appearance of it; he remarked moreover that all the plants had it not equally,
that

* *Christ. Lud. Gersten Tentam. Francof. 1733.*

that on some it was very abundant, whilst he could scarce perceive it on others.

We shall not relate a great number of experiments, that were made by Mr. Gersten, altho' some of them are extremely curious; but the reader will see by those which we shall give an account of afterwards, that it was necessary, in order to draw certain and exact consequences, to have a knowledge of several facts, the discovery of which was reserved for Mr. Mussichenbröek; who after having verified the greatest part of Mr. Gersten's experiments, at first submitted himself to his opinion, but he hath since changed, and is fixed in acknowledging several sorts of dews, of which one more dense than all the others rises from lakes, rivers, and marshes.

It is this the rising of Thetis is compared to by Homer,

Καρπαλίμως δ' ἀνέδν πολὺς ἁλὸς, ἢ τ' ὁμίχλη·

A second perspires from plants and from the earth itself.

This is what I take to be meant by Virgil in the word halantes,

Invitent croceis *halantes* floribus horti,

And Ovid particularly applies it to the earth,

—— repetebam frigus, et umbras,

Et, quæ de gelidis *halabat* vallibus, auram;

And a third falls from *above*. Mr. Muffchenbröck founds the existence of *this last* on a great number of experiments which he hath made upon the leaden terras of the observatory at Utrecht ; it was not possible that any vapour should rise from this terras of lead, yet several bodies which he exposed there received dew on their upper surface ; from whence Mr. Muffchenbröck concludes, that there is really a dew that falls.

Several observations and experiments repeated very often, and which always proved uniform, leave no longer any doubt with me concerning the nature of dew, at least with regard to its *rising* or *falling* ; and I think one may be satisfied that it *rises* only from the earth and plants ; that this moisture or this vapour consists of an infinite number of little aqueous globules extremely light with which the air is loaded, and which it carries with it whithersoever it is driven by its fluctuating motion : Thus the bodies which are met by this aqueous vapour, receive it in all the parts of their surface, and are *immersed* all at once.

In the experiments of Mr. Muffchenbröck where bodies exposed on the leaden terras received dew on their upper surface, it is true, the moisture did not come from the terras, but it rose from the earth, and from the plants round about ; it came successively, and by the vehicle of air to the terras of lead ;
it

it was afterwards diffused through all the air that was above this terras, and fastened to the bodies which were exposed to receive it : If Mr. Musschenbröck had attended to the *time* when the dew began to be perceived on the terras, he would have remarked, that it was sensible a long time before at the foot of the tower, on whose summit he made the observation ; I even doubt not but if he had raised a mirrour or a piece of glass to six feet above the leaden terras, he would have found drops of dew attached to the inferior surface of the glass, as soon, and in as great quantity as on the superior surface : This I cannot doubt of from experiments I have made along the different heights of the steps of a ladder. I placed two ladders opposite each other, joined at their tops, spreading wide asunder at their bottoms, and so high as to reach thirty-two feet. To the several steps of these I fastened large squares of glass, and set them in such manner, that they should not shade or over-hang one another. It was plain that if the dew descended without first rising, the top squares must be first wetted, and that on their upper surface ; but on the contrary, if the dew first ascended from the earth, the bottom surfaces of the lower panes must first receive it : And thus in fact it happened ; the lower surface of the lowest piece of glass was first wetted, then its upper surface ; then the lower surface of

the pane next above it, and so on gradually till the whole was wetted to the top of the ladders.

But in order to assure myself more certainly, and to leave no room for doubt on this subject, I have lately repeated the same experiments at Paris upon a leaden terras. I suspended a glass upon a wooden frame, raised only two feet, and I constantly found drops of dew pretty nearly in equal quantity on the under side as on the upper side of the glass; which is different from the former experiments that were made in the country, where the inferior surface was always sooner and more abundantly moistened than the superior; but the reason of this difference is very evident, for in the country the aqueous vapours rose directly from the earth, and fastened themselves to the under surface of the glass, but it is not so on a terras of lead and surrounded by lofty buildings; in this case, the air that is loaded with moist vapours cannot arrive thither but by long windings and a very irregular fluctuation, so that there is no more reason why it should fasten itself to the under than to the upper side of the glass; or in other words, the vapour doth not rise there perpendicularly as in the country, but is driven in a lateral direction.

Notwithstanding all I have been saying, I do not pretend that there is but one kind of dew, and deny not but there may be a dew
whose

whose parts are gross enough, and have sufficient weight to fall to the ground; yet I think in that case it would be *visible*, and form what we know by the name of *fog*: But our inquiry is at present of that kind of dew, so stiled by all, which is imperceptible to the eyes, and becomes sensible only by wetting those bodies that are exposed to the air in the night.

I thought it necessary to begin this memoir by the general notions I have been giving concerning dew; and altho' my principal object was not to make inquiries on the nature of dew, yet I conceived it proper first to establish what it is, and from whence it comes, before I pass to what I have to say with regard to the nature of the bodies to which it attaches itself; this is what we are now going to examine, and I shall relate facts which I may justly declare new and extremely surprising, that have been communicated to me by a letter, which Mr. Musschenbröek did me the honour to write to me the 16th of July 1735. I have with great care verified the experiments contained in that letter, and I have added some others to them since, which support and confirm those of Mr. Musschenbröek, as will be seen in the course of this memoir.

I shall begin with giving an extract from the letter I have been speaking of.

Mr. Muffchenbröek exposed on the leaden terras of the observatory at Utrecht different bodies to receive the dew. The result of an experiment so simple was much more singular than he expected or could possibly be foreseen, for some of these bodies received the dew very abundantly, others received but little, and lastly, others continued as dry as when they were exposed. The bodies which received the most dew were a varnished board, a table of slate, a china dish, a crystal vessel, a piece of talc, and lead painted white in oil. Those which received none at all were the blue stone of Namur, a silver-gilt dish, one of polished silver, another of silver whitenened with a lye of salt of tartar, a pewter dish polished, and another rubbed over with pumice-stone, a plate of brass, and another of iron, both polished, and a mirrour of metal.

A board painted in oil, and placed aside all those other bodies, had received the dew, but it did not run as it did on the vessels of crystal and of china-ware, because it was stopped by the inequalities of the painting, the same thing happened to boards of fir, which nevertheless appeared sensibly moistened. Unpolished brass and unpolished iron had on their surface very distinct drops of dew, but nothing was more singular than a china dish, in which there were six pounds of quicksilver, for you saw a stream of liquor
on

on the edges of the dish, round the mercury, and not the least appearance of any on the mercury itself; white cloth and scarlet cloth had amassed a considerable quantity, as was discoverable by the weight, but they had it unequally. This difference shall be examined afterwards.

To shew that there is a dew during the whole course of the night, we shall only mention by the way, that a piece of scarlet cloth, of four feet square, had increased in weight the 1st of June at 9 o'clock in the evening 7 drams 24 grains, at 11 o'clock 12 drams 10 grains, at one in the morning 15 drams 18 grains, at 3 o'clock 18 drams 30 grains, and at 4, 21 drams 25 grains. It will be easily thought, that this increase doth not always keep the same proportion, and that one must expect to find as much variety in dew as in rain, winds, and other meteors.

Mr. Musschenbröck, in a second letter of the 27th of August 1736, communicated to me some other observations that he had made on the same subject. He exposed to the air, on a plate of copper polished, and with all the precautions that might be expected from his sagacity, pieces of 8 inches $\frac{1}{2}$ square, each of different leathers and of different colours, such as * Spanish leather red, yellow, blue, and black; shamoy, Russia leather, and white calfs leather. Having repeated twice the

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same

* Marroquin, goats leather dressed with galls.

same experiments, the result was, that neither the thickness of leathers, their hardness, their dryness, nor their suppleness, made any alteration as to the quantity of dew; the red Spanish received the most, the black and the blue received no more than about half the quantity of the former.

Mr. Musschenbröek adds, that having been informed by Mr. Gray's experiments, and mine, of the differences which resulted from *colours* with regard to electricity, he was willing to be assured whether *they* had not an effect upon dew, considered as colours; that is to say, as reflecting a greater quantity of rays of one sort than of another; and for this purpose he took several little porringers of wood alike and perfectly equal, and besmeared them internally with different colours tempered in linseed oil. The first porringer was with pure vermilion, the second with orpiment, the third with ceruse, the fourth with verdgrease, the fifth with Prussian blue, the sixth with Florence lacca, the seventh with the black of smoke, and he added an eighth which was not painted at all. All these porringers were placed on the plate of copper, and with the same precautions as the leathers had been; the dew fastened on them very unequally; it was on that which was not painted that it fell in the greatest quantity, and the white and yellow received the least; that which was done over with ver-

million

milion had but a very little more, and yet the *red* Spanish leather had attracted more than all the others: It appears therefore from these experiments, that it is the materials alone which serve for colouring, and not the colours in themselves, that occasion some bodies to receive the dew more abundantly than others. We have observed the same thing with regard to electricity, and altho' Mr. Gray imagined that he saw differences resulting from colours, I have proved by several experiments, that they resulted only from the ingredients of different sorts which entered into the composition of the dye or colour.

This fact being well established by the observations we have been relating, I believe that the difference in the quantity of dew can no longer be attributed to the *figure* of bodies that receive it, as Mr. Gersten hath thought; but the experiments on which he rested his opinion, are not sufficiently decisive, as may be seen in the work which we cited above: And the experiments which we are going to mention plainly shew, that this inequality arises only from the difference of substances that are exposed to the dew.

The first thing that I did, after having received the letter of Mr. Mussichenbröek, was to repeat the experiments which it contained; and for this purpose I exposed to the dew vessels of silver, copper, crystal, china, and
several

several other substances of which it is useless to make an enumeration ; because all the surprise of the observation consists in this, that there is a body which doth not receive the dew, whilst another that is placed close by it receives the dew abundantly : This fact once proved, it will no longer be surprising, that there are bodies which receive it some more and others less copiously.

Having therefore found by observations, repeated a great number of times, and never contradicted by any experiment, that vessels of crystal received the greatest quantity of dew, and that those of any metal whatever, being well polished, received not a drop ; I was willing to see whether this so singular an effect might not arise from hence, that the dew, wetting the glass more exactly than the metal, adhered to it more closely, inso-much that it evaporated sooner from the metal, and left no trace behind it in the morning, altho' it had really fallen in the night as much on the one as on the other : For this purpose I caused a great funnel of crystal-glass to be made, which had 14 inches $\frac{1}{2}$ diameter, and another of pewter well polished of the same size, and perfectly alike in all its dimensions ; I placed each of these funnels on a trevet of iron varnished, raised from the ground a foot and half, and I placed underneath these funnels vessels alike and equal, that is to say, one of crystal underneath
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the funnel of crystal, and one of polished pewter underneath the pewter funnel; these little vessels had a narrow neck, in order that the liquor, which might flow down, should not be able to evaporate, and the neck was wider in its upper part, in order to receive the extremity of the funnel. I placed these funnels thus prepared with their trevets, sometimes on a meadow freshly mowed, and sometimes on bare earth. It would be a tiresome detail to give an account of each observation which I repeated several days successively, and in different seasons of the year; it is sufficient to take notice that I have often found in the vessel which was placed underneath the crystal funnel seven drams, and sometimes more than an ounce of liquor, and that there never was so much as a single drop underneath the pewter funnel, when there had been nothing but dew in the night; for it may well be imagined, that when it rained, there was water in both vessels, and this circumstance proved the great equality of their surface, there not being sometimes 10 grains of difference upon 4 or 5 ounces of water which I found in the morning in the vessels underneath.

I have often been to visit my funnels at different hours of the night, I found almost always drops more or less big upon that of crystal, and never the least appearance of moisture upon that of pewter; from whence

it must be concluded, that what is commonly called the *falling dew*, doth not in reality fasten itself to the pewter funnel.

I do not say the same with regard to the moisture which rises from the ground and is properly speaking intercepted; for one finds sometimes a great quantity of moisture affixed to the under part of vessels of metal, and when I have turned topsy turvy my funnels in order to receive the portion of each, I have often found a moisture hanging even to the interior point of the funnel of pewter; but this requires a particular inquiry, for I am inclined to think, with Mr. Gersten and Mr. Musschenbröek, that the moisture which rises from all plants is not of the same nature; and perhaps it would not be an useless employment to collect separately the vapour which rises from plants, whose properties are well known; it may have something of the nature of those plants, and under that form be of some use in medicine. The method of collecting it by glasses placed over the plant is extremely easy, and by this means we may get the distilled waters of plants from nature's chemistry.

In order to be assured whether the difference was always the same, in all circumstances, between vitrified substances and metals, I placed a China saucer in the middle of a silver plate, and immediately on one side I put a silver vessel very like the saucer upon a China plate;

plate; the event was intirely conformable to what we have hitherto seen; the China saucer placed on the silver plate was all covered with dew, whilst the plate which spread four inches round had not a single drop, and the China plate received the dew as usual, whilst the silver vessel that was in the middle was as dry as when I exposed it.

I was willing to see whether a China saucer placed in this manner upon a plate of metal, did not receive more dew than it would have done if it had been exposed quite *alone*; for it is possible, that what doth not fall upon the plate may be *diverted* towards the China, and by that means increase the quantity of that which would fall there naturally. For this purpose I took two large crystals for a watch perfectly equal, which I exposed, the concave part outwardly, the one upon a silver plate, and the other upon a China one. I had surrounded with a ferrel of polished silver, that which was placed upon the silver plate, in order that there might not be any dew attached to the convex part of that crystal, and that by this means the experiment might be as exact as possible. I exposed for several days successively these two crystals disposed in the manner I have mentioned, and I always found five or six times more dew in that which was on the China plate than in the other. There happened even something very singular with regard to that

that which was on the silver plate, for the little dew, that was in the inside, was only towards the center in small drops, and there was round the border a space of five or six lines intirely dry, and towards which the drops continually decreased in bigness; as if the silver ferrel which surrounded that border, but without hanging over it or covering it, had repelled the dew and hindered it from fastening to that part of the glass which was immediately contiguous: I have repeated this experiment more than thirty times, and always with the same success.

I shall now mention another fact which hath something still more singular, and confirms what we have been observing. I made a plate of six inches square, and a line in thickness; it was formed of two other plates six inches long *each*, and three inches wide, joined to each other *sidewise*, and fastened together in this manner by two little grooves; the one of these two half-plates was of polished brass, and the other of glass. I placed sometimes upon the ground, sometimes on a board, and sometimes on other bodies, this plate composed of two substances so *heterogeneous*, at least relatively to dew; the reader will easily imagine from all we have seen before, that the part consisting of glass was covered with dew, and that the brass part had none; but we are going to observe something more, for having placed upon this plate
sheets

sheets of glass which rested at one end upon the brass, and at the other upon the glass, this last end received the dew very abundantly, and the end which rested upon the brass received none at all. As this experiment gave me no difficulty, I repeated it almost every day that an opportunity offered, and never was there any difference in it, except that it happened sometimes that the sheet of glass did not bear at its extremity upon the part consisting of brass, it was sometimes a line distant, and in this case there did fasten some drops upon that end of the sheet of glass, but never on the part which touched immediately the brass, as if the metal was surrounded with an atmosphere which kept off the dew, not only from the surface of the metal, but also from all that was within the sphere of its activity.

I shall not speak of what happens, when instead of dew there is an hoar-frost or a fog; this detail would be too long, and moreover I have not yet a sufficient number of observations to be able to say any thing very positive on this subject: I shall only mention at present, that it is not with respect to, a fog in the same manner as we have seen with regard to the dew, that the fog falls upon the funnel of metal, but always in much less quantity than on a funnel of glass: with respect to the hoar-frost, it appears upon the one and the other under very different forms,
and

and which deserve our attention ; we shall say something of it on another occasion.

Memoirs 1736, pag. 352.

A curious
account of
a natural
ice-house.

An account hath been transmitted to Mr. Dodart, that there is at five leagues distance from Befançon a great cavern scooped in a mountain, whose top is loaded with oaks and other great trees ; the entrance resembles the gate of a city ; the vault is extremely high ; you see clearly throughout the whole, and the inside is a vast square saloon, whose pavement is of crystal ; there is often ice in it four feet high, and there are besides that large pieces that hang from the vault in the form of festoons.

In winter the vault is full of thick vapours, and there flows at the bottom a little rivulet : It hath been remarked, that after having cut some of the trees that are at the entrance, the ice hath been much less abundant for a long time after.

They come from all parts to this ice-house with carriages and mules, which transport provisions of ice throughout the whole province ; yet the ice is never exhausted, one day of hot weather reproduced more than had been carried away in a week.

The person who hath sent this account went thither himself attended by a great number of people : they observed to him that there were then fogs in the grotto, which

which they said was an infallible sign that there would be rain the next day, as in reality happened. The country people round about never fail to consult this singular kind of almanack, to know what weather they shall have in the different works they undertake.

Mr. Billerez, professor of anatomy and botany in the university of Besançon, thought that he had discovered the cause of this phenomenon, by observing that the earth in the neighbourhood, and particularly above the vault, is full of a natural sal ammoniac.

But Mr. des Boz hath sent word to the Academy that he had taken four journies to the grotto in the four different seasons of the year; that he had found by the thermometer the air much more cold in the grotto than without, but still more warm in the warmer seasons, agreeable to the usual rule in cellars.

The cause of the great coldness that is constantly found in the grotto, tho' less in summer, is very natural. Its mouth or opening, as Mr. des Boz observed by the compass, is directly exposed to the north north-east, and in that line looks towards a streight of mountains, which lets only a very cold wind pass through it, and whose coldness it increases by contracting its passage. A jutting rock covers likewise a part of the mouth of the grotto, and defends it from the external air; except a little meadow that is above this rock,

all the rest of the mountain is covered with great beeches, the foot of which is invironed with shrubs or brambles, and consequently the rays of the sun can scarce penetrate to the earth, and still less to the rock that is underneath, and which makes the vault of the grotto. Persons worthy of credit had assured Mr. des Boz, that since they had cut down some of those great trees above the grotto, it had not furnished so much ice as before.

In order to try whether there were any salts that might be the cause of the congelation, Mr. des Boz dissolved and evaporated intirely a great quantity of the ice of the grotto, and there remained at the bottom of the vessel little gravel stones, which had no other taste than that of crabs eyes reduced to powder.

But there are not in the grotto any springs of water, as some have advanced. The ice arises only from rains and melted snow, which straining by little and little through the ground and down the crannies of the rock into the grotto, there congeal as well to the vault as to the sides, and this, almost certainly in every season of the year how warm soever it may be.

History Tom. 2. anno 1686, pag. 2. History 1712, pag. 22. History 1726, pag. 16.

Vid. The account of the icy cave of Selicze in the Philosophical Transactions abridged by Dr. Martyn, Vol. viii. Part ii. pag. 662.

Amongst

Amongst several pieces of natural history which his highness the duke of Orleans hath received this year, and which have been sent him by Mr. Lieutaud surgeon, and by Mr. le Juge counsellor to the superior council of the Isle of France, there are two kinds of nests § made by caterpillers, the construction of which is singular enough to deserve to be described. The nests of one kind are each the work of a single caterpillar; those of the other are each the united labour of a numerous family of another sort of these insects. The caterpillers that build the first kind of nest introduce in the frame of it little pieces of wood ranged in a certain order: But there is nothing *foreign* in those of the latter kind; they are composed of pure silk, and of a silk that is pretty strong. We know, by the memoirs of Mr. de Reaumur upon insects, several nests of these animals, that in that particular resemble those we are speaking of. There are for instance divers caterpillers that strip themselves of their hairs, in order to strengthen the sides of their silken balls; or who *case* them with earth or other materials, such as little pieces of dried leaves: One might find likewise examples, and which would come still nearer to our nests, in the sheaths of several aquatic moths, that are in part composed of shells, grains of sand, and dried leaves, or little sticks, ranged in order

A description of two remarkable kinds of nests made by caterpillers, by Mr. Guettard.

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and

§ These nests are from Fort-dauphin, in the island of Madagascar.

and with a kind of symmetry. The nests of common caterpillars, whose construction hath been so well described by Mr. de Reaumur, may likewise be compared to those I propose to speak of; but altho' these nests are very artfully made, and that they seem to require a great number of *designs* in the animals that build them; yet it may justly be said that one single property which those have that are the subject of our present inquiry, and which is wanting to the others, appears to require a much greater fore-sight and precaution in the caterpillars that formed them. These nests are suspended to branches of different trees, and in *that* resemble the nests of several sorts of wasps. (*Vid. Vol. I, pag. 204.*) It was necessary for the caterpillars, that includes herself singly in her nest, to take a proper precaution against the *rockings* which might happen from the agitation of the air: The caterpillars that croud together in vast numbers in the same nest, had the fore-going inconvenience to guard against, and moreover, that which might happen from their number if the silken cods had not such or such a form, and if they were not fastened one to another. We shall see, when I have given the description of these two kinds of nests, that there would result an inconvenience still greater from the mere construction of these nests, if the caterpillars did not prevent it, altho' it

was

was requisite at the same time to be prevented under certain cautions.

These different considerations engaged me to give an account of these nests, which have not hitherto, I believe, been described by any author.

Those which are built by a single caterpillar, are more *long* than *wide*; they have the form of a spindle a little less elongated in its upper part than in its lower: this latter terminates with a kind of cylindrical tube formed by the contraction the nest suffers in that place; the other end hath a collar, at the extremity of which there is a ring that passes into the branch to which the nest is suspended. This nest is, properly speaking, composed of three planes; the first is formed of a filken web that covers externally the whole nest, *fig. A*; the second is composed of an assemblage of dried leaves, or little sticks, *fig. B*; the third exhibits the cod which is of silk, *fig. C*. The dried leaves or little sticks do not appear externally, as they do in the sheaths of aquatic moths; it is easy however to be assured, that there must be in these nests some other material besides silk; they appear to have a sort of bulging out on the one side and the other, which plainly indicates some other substance than silk. If you lay them *bare* by lifting up the filken web that covered them, you see that these little sticks are wanting on that part of

the nest which forms the inferior tube, and in the upper part for a third or thereabouts of the nest.

When you have removed these little sticks, there remains a filken cod *fig. C.* of a dirty white, and striped with brown and transverse marks, which have been occasioned by these little sticks. The internal sides are smooth and appear bedawbed with a gummy or resinous matter; it issues from the sides of those threads of silk that form a kind of net, in the midst of which the chrysalis is placed: (*Plate 8th, D*) the internal orifice is stopped up by a considerable mass of silk of a loose texture, and easy to be divided. This cod thus stripped of its little sticks, and the outside web, is at most no more than the half of the size of the nest: The smallest of these nests are in their greatest diameter about an inch and half, and the largest of them about two inches.

The nest being suspended, there is danger that the motions, which it must suffer from the agitation of the air, should occasion the chrysalis to be tossed to and fro; this inconvenience is removed by the net that *surrounds* the chrysalis, and to which it is moreover fastened by two hooks that it hath at its posterior part; thus fortified, it follows all the motions that the cod can receive,

And in the visitation of the winds
is not carried sometimes on one side, some-
times

times on another, and in a contrary direction to that of the cod ; which must often have happened without this precaution.

If the chrysalis had reason to fear that its state of repose should be disturbed by repeated shocks, it had still more to apprehend from a species of insects, that is destined to live under the form of a worm at the expence of other insects, and to feed upon them, I mean the ichneumon-flies : It is well known, that the females of these flies deposit their eggs in the nests of almost all insects, that they place them oftentimes in the body of several, and even in their eggs ; it might therefore easily happen, that the caterpillar of the nest we are speaking of would not be able to avoid so cruel an enemy, if she had not the art to close the internal orifice of her cod : This orifice therefore is *stopped up*, and it is *so* likewise with certain precautions : That kind of filken tow which I mentioned, is the stopple that the caterpillar makes use of, and she makes it big enough to be able to resist intirely the efforts of an animal, so considerable as that which might enter into her cod, the *external* opening of which, *that* is not closed, is more than two or three lines ; and which might become still bigger by the ease that the animal would find of dilating it in forcing the sides of the tube, which easily yield, notwithstanding their close texture. The caterpillar prevents this by making the

filken mass *large* enough to stop up exactly the internal orifice, and *long* enough that she might not be herself within the reach of the animal, who could easily arrive to the limits of this opening.

But if this kind of diaphragm or valve defends so well the chrysalis, is there not reason to fear, that it should become likewise an invincible obstacle for the butter-fly when she would sally forth from this cod? Although the texture of this stopple should be as strong as that of the cod, altho' it should be even much stronger, there is no doubt but the insect would find the means of taking flight; nature would have taught her some art to open it, or would have furnished her with some liquor for softening it, and rendering it more easy to pierce, as she hath done for several other species of insects. It appears however, that all the contrivance of this insect consists in separating and dividing the filken tow, which must be easy for her to penetrate, tho' it may and ought to be of difficult access to other insects that only seek to annoy her: Her sallying forth is facilitated also by the situation in which the chrysalis places herself, with her head downwards, and sunk a little in the tow.

Although I had under my eyes in the second nest which shall be described afterwards, and which is the habitation of a numerous family of caterpillers; altho' I had, I say,
an

an example of a nest suspended and destitute of the plane of little sticks, with which *this* is surrounded, I have always been inclined to believe, that this plane might be made in order to defend the chrysalis still more; and to shelter it from some insults. We have learnt from the memoirs of Mr. de Reaumur, what taste the birds of our fields, the gold-finches for instance, and the sparrows, have for the common caterpillars; these birds tear and pull to pieces the nests, in order to draw out the caterpillars that are included in them, and on which they feed particularly in the winter. These birds cannot without doubt so easily destroy the nests of the caterpillars we have been describing, fortified as they are with little sticks closely connected together.

It is not therefore allowing too much to our insect, to say, that it hath the skill to guard itself against enemies much stronger than those we have just now mentioned, such as some birds may be, that with their strong and robust bill would tear to pieces a nest made solely of silk, or, like that of the common caterpillars, fortified only with leaves that quickly grow dry and wither, and in consequence become more easy to be torn to pieces; but who may very well be supposed to make vain efforts against a nest defended as *this* is, particularly when *suspended*, and by that means giving less hold to these animals, in not furnishing them with a solid support

support on which they may place themselves. This being so, it must be acknowledged that this caterpillar hath the superiority in foresight over those that build the other kind of nest, which may be exposed to the same dangers, and consequently the nymphæ that are included in it.

An insect that can take such caution against so many dangers, is not likely to be unskilful in a point so essential as that of the *suspension* of its nest. Since the nest was to be *thus* fastened, it ought to be so in such manner that it should not easily be loosened, and yet be complying with all the agitations to which it might be exposed: now this is what the animal hath had in view; if the *bold* had been too loose, the nest might have slid along the branch, and oftentimes have fallen to the ground; if *it* had been too tight, the nest might have been dashed to pieces by the force of the wind. Instructed as *we* are by social converse, I fancy that we should not be able to act better than this insect, and that we could not carry our attention further. If we had such a body to suspend, we should do it without doubt, by means of a ring which should exactly embrace the body to which we would fasten it. This is what the caterpillar doth, and if there be any differences they only turn to the advantage of her industry.

When

When the nest is intirely finished, and the caterpillar hath *taken the veil*, no more to go forth under that form ; it may be said that she hath done all that could be expected, in order that her state of retirement should not be disturbed.

But how does the caterpillar set about making the nest, in which there enter so many views and so much cunning ? We have endeavoured to discover these views by *unbuilding*, as I may say, the nest, and placing all the materials before our eyes : let us now endeavour to represent to ourselves the caterpillar at work, and employing in the construction all the artifice she must necessarily have.

We can have no doubt concerning the part by which she must begin, it is certainly by the ring : she may make it by turning several times round the branch, and leaving on it each time a thread ; or, keeping fixed to one place of the branch, she may incline the forepart of her body sometimes on the one side and sometimes on the other, curving herself sufficiently to embrace the branch, and make the tour of it ; which cannot be difficult for a caterpillar so big as that must be which makes so considerable a nest, and whose chrysalis is so large.

The ring being made, it will be easy for her to spin a bundle to which the nest may be suspended ; she hath nothing to do but to
fasten

fasten in all the inferior part of the ring threads at first left free ; and which she may afterwards connect by transverse threads. I imagine she *then* quits in part the branch to which she had hitherto been confined, and only holds to it by her hinder legs ; that she advances afterwards on the bundle, where keeping herself grappled by scaly feet, she may easily begin the upper part of the nest, on which she will advance in proportion as it increases ; and when the extent given to it shall be such that she can lodge herself in it intirely, she then will labour to form what may properly be called, the nest.

Hitherto the caterpillar hath been obliged only to draw from her own fund a matter, with which she is amply provided : but she must now know where to find those little pieces of wood, of which we spoke in the description of the nest ; she must, and which requires still more cunning, know how to raise them to that part of the nest which is already made, and likewise how to place them exactly : all this will be done without much difficulty ; the caterpillar will descend to the *ground* by means of a thread which she will fasten to the part of the nest already made ; *there*, without quitting her thread she will search for some little stick, and probably she will not be a long time without finding some ; possessed of one of these sticks, she will hold it firmly clasped between her hinder

der and her middle feet, and lay it along her belly; afterwards she will mount up again thus loaded, by the thread that served her to descend; as soon as she is arrived to the top of this thread, and consequently to the part of the nest where it is fastened, she will fix in that place the stick which she bears, and lay it horizontally. I imagine she fastens the little stick by means of several threads which she can multiply as often as it is necessary, in order to make a sufficiently strong cordage. In mounting up again she winds into a skain the thread, by which she was suspended, between her four hinder-most and scaly feet, and makes of it a little clew, which she deposits on the nest, and after having deposited *that*, she lays and adjusts the little stick. Which done, the caterpillar must repeat the work again, in placing another; she must employ a good deal of time in going to fetch these sticks; each stick requires a little journey: I have counted in a nest of the least size an hundred of these sticks, and thus there must be as many *goings* and *comings*, which will be more or less long, according as the branch where the nest is fastened is more or less elevated.

When all the sticks are placed, she must then stop up the nest; if the caterpillar could find the means of doing it with such little sticks, which would not be an easy matter, yet the butterfly would have a great deal of difficulty

difficulty in breaking down this dike, or rather would not be able to effect it; if she inclosed herself *immediately* with a stopple of silk, or by a web, perhaps she might be too much exposed in the chrysalis state to some of her enemies: What then doth she do? She lengthens her cod in such manner that it forms a tube nearly cylindrical; by this means the opening is already much diminished, the chrysalis must be less within the reach of the animals from whom she hath any thing to fear, and it must be more difficult for them to introduce themselves into the nest through this tube, than if the nest had been destitute of it: Some however might still be able to force this obstacle, their smallness might give them admission: to such our caterpillar opposes the filken mass of which I have already spoken, it closes exactly the internal orifice of the tube, and by this means defends the chrysalis from any surprise; and if the chrysalis be notwithstanding sometimes devoured by the worms of some ichneumon-fly, the fly must have deposited her eggs before the cloister of the nest was finished, or have laid them on the caterpillar herself, before she retired to her cell. It must be owned, that there hath been already much silk employed in this nest, and that a caterpillar which could furnish it all, must have been extremely well provided; yet she can still find a store, and even in a pretty large

large quantity : I imagine that there remains to her enough for *strengthening* the internal sides of her cod, and that she *does* it by adding thereto some lays of thread, and perhaps smearing them with a resinous or gummy matter, considering the slipperiness and the shining of these sides. Lastly, she must spin moreover that net in the midst of which you find her placed, and by means of which she is exempt from all the tossings that she might otherwise suffer.

I shall now proceed to examine the nest which is built by an entire family of these insects.

This nest agrees in several things with the preceding ; it is like that suspended to the branch of a tree ; like that it hath at bottom a tube whose external orifice is open, and the internal closed by a silken tow. This last nest differs from the former in these particulars, that it hath not the *figure* of a spindle, but *that* of an inverted cone, that there enter no little sticks into its construction, that it is intirely composed of silk, and the work of a great number of caterpillers, who must, as I think, make it not as the preceding at the time precisely of the metamorphosis into nymph or chrysalis, but some time before ; it serves them probably till that time as a place of retreat when they cannot continue abroad. I fancy that they agree in this particular with the *processionary* caterpillers, because, like them,

Vid. Plate
9th, A.

them, they keep themselves separate upon trees, till the time is come for their undergoing a metamorphosis; till then they live in solitude, and when the time is come for social union, they labour in concert on this *public* edifice: It cannot however be so *expensive* a building as the former nest, which must require much more time, and furnishes much more silk. If we compare these nests together, the first will be at least the fourth or fifth part of the second; this last is in one of its greatest diameters, that is to say, at its base, half a foot long, and a little more in one of its greatest sides; that is to say, from the inferior extremity of the tube, to one of the ends of the great diameter of the base: The least diameter of this base is about two inches. The length of the former nest was three or four inches, upon two or thereabouts of breadth in its greatest diameter.

Although the caterpillers of this second nest are not so industrious as the others, yet they deserve perhaps no less our inquiry into their manner of proceeding. This nest is without doubt the work of the whole family, but which labours in different companies; one party of these caterpillers ranged on the branch to which the nest will be suspended, works in forming the string by which it is held, pretty much like the caterpillar of the preceding nest: I say *pretty much*, because in reality it is not so regular a ring as the former.

mer. The silk perhaps hath been employed with less art, but then it hath been well employed for common safety; not only a large branch, but likewise several little sprigs are caught and surrounded by this string, which by that means becomes certainly more firm and secure: This *fastening* having been thus begun by a party of the caterpillers, another party comes to increase its solidity, and the whole community contributes to its perfection.

This important part being finished, each caterpillar concurs in forming the body of the nest; she can easily work on it by standing on that part which is already finished. In order to understand clearly how this may be executed, let us suppose the *fastening* entirely completed, it will then comprehend all the base of the nest, a party of caterpillers will place themselves there at their ease as on a branch, marching thither either in troops together, or in file one after another, and each will enlarge it a little by spinning horizontally. When the nest was first begun, the caterpillers, being well provided with silken matter, could make a considerable piece of the nest every time that they worked; by degrees that matter is exhausted, each caterpillar by consequence can only apply a piece less broad and long, the nest will by this means be contracted, insensibly decrease, and consequently take a conical figure.

The construction of the nest having been supposed to be finished a good while before the time of the metamorphosis of the caterpillers is come, it is easy to imagine, when that time comes, how the net and the cods are spun: The *caterpillers* having immured themselves in the nest, with design no more to leave the cloister under *that* form, must think of giving it a little more consistence, and close the internal orifice of the inferior tube; this being transacted *in common*, each must take her place upon one of the sides of the nest, there spin the filken mass that surrounds each cod, and form the net that is to preserve her from tossings, which is only a continuation of the filken mass that constitutes the cods.

EXPLANATION of the FIGURES.

Plate 7th.

- A. The nest intire, suspended to a little branch of a tree.
- B. The same nest, from which the external filken web is removed, in order to shew the arrangement of the little sticks, which defend externally the cod where the chrysalis is included.
- C. The cod stripped of these sticks.

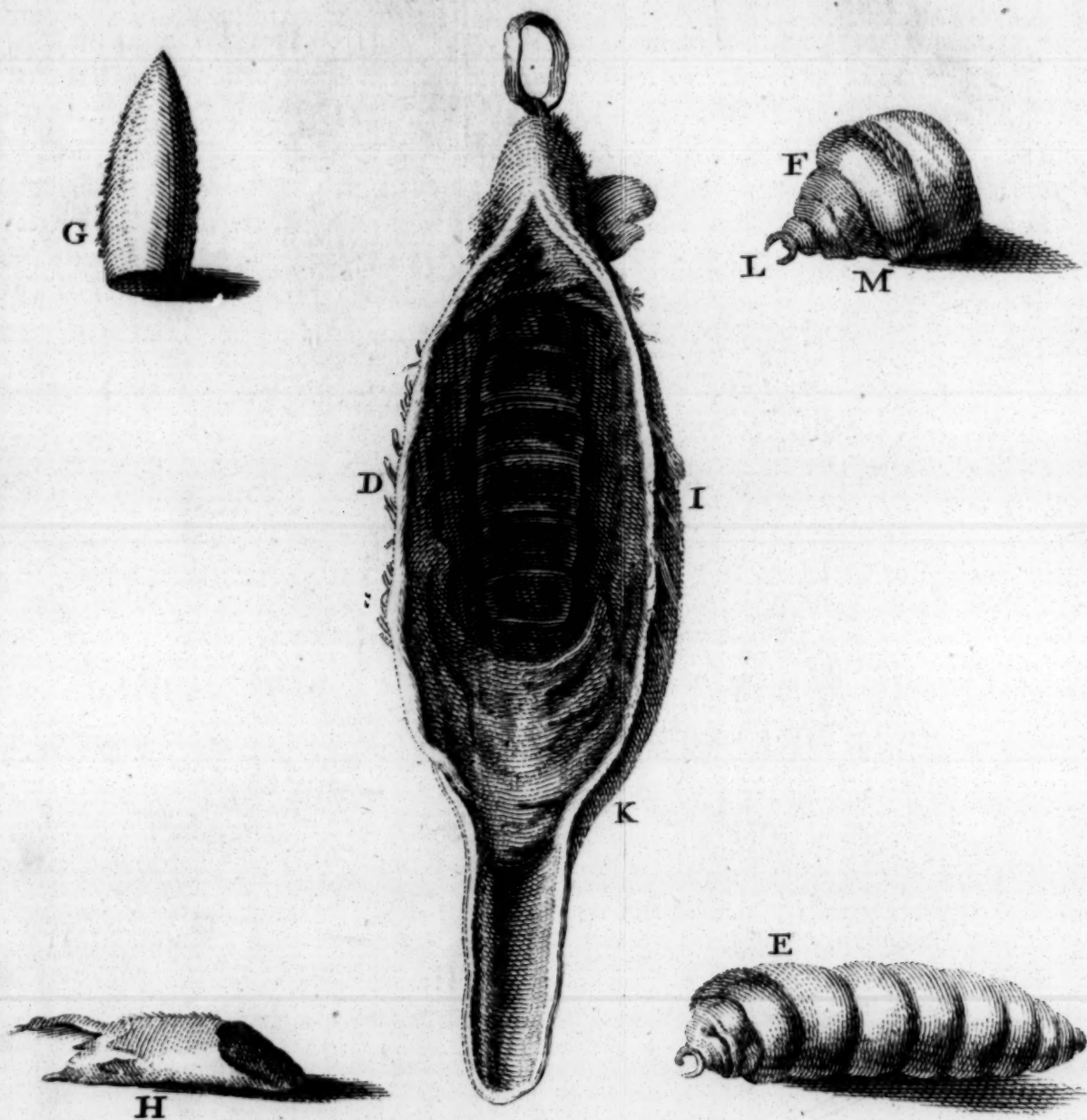
Plate 8th.

- D. The same cod opened through its whole length, to shew the chrysalis I, and the
filken

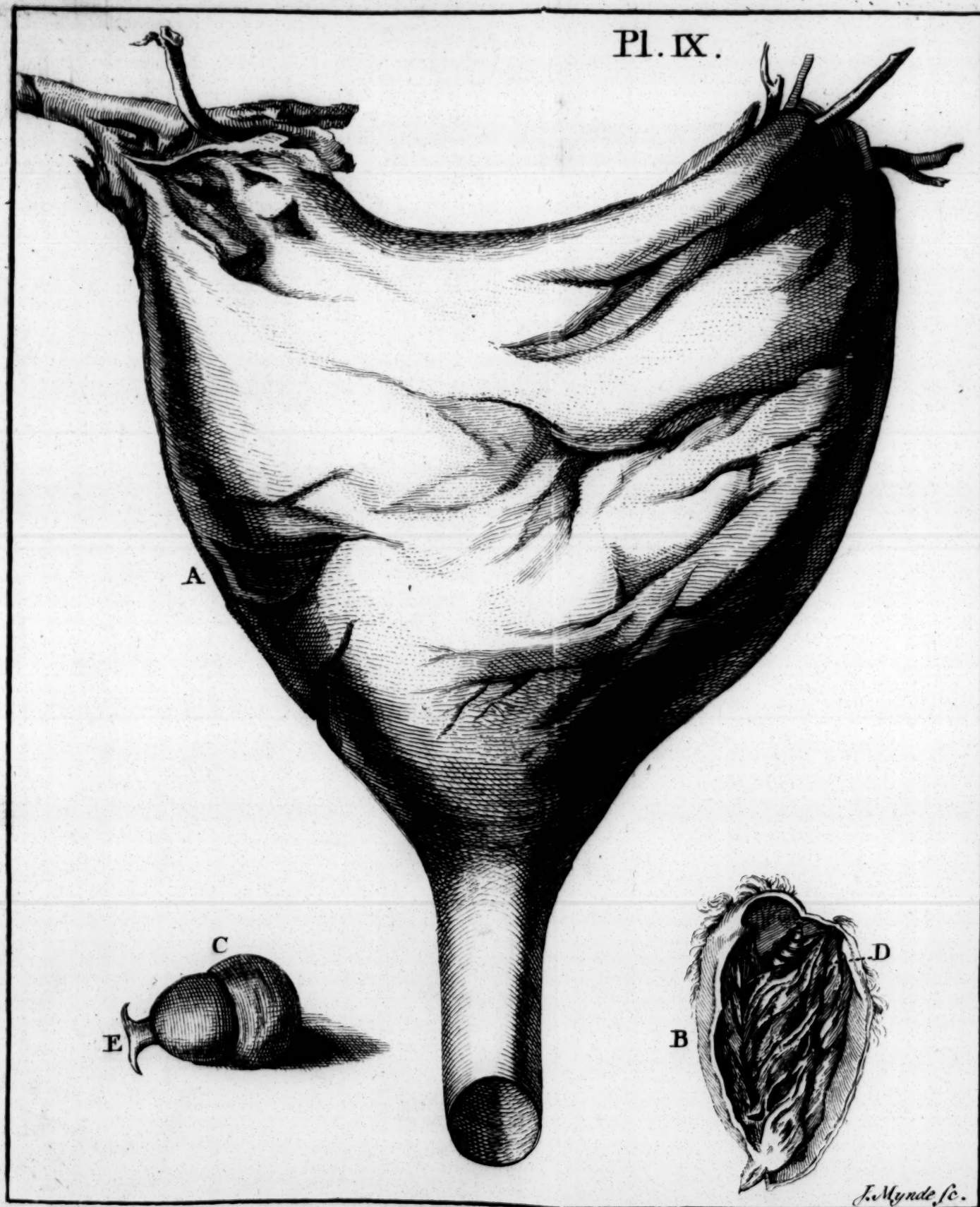
Pl. VII.



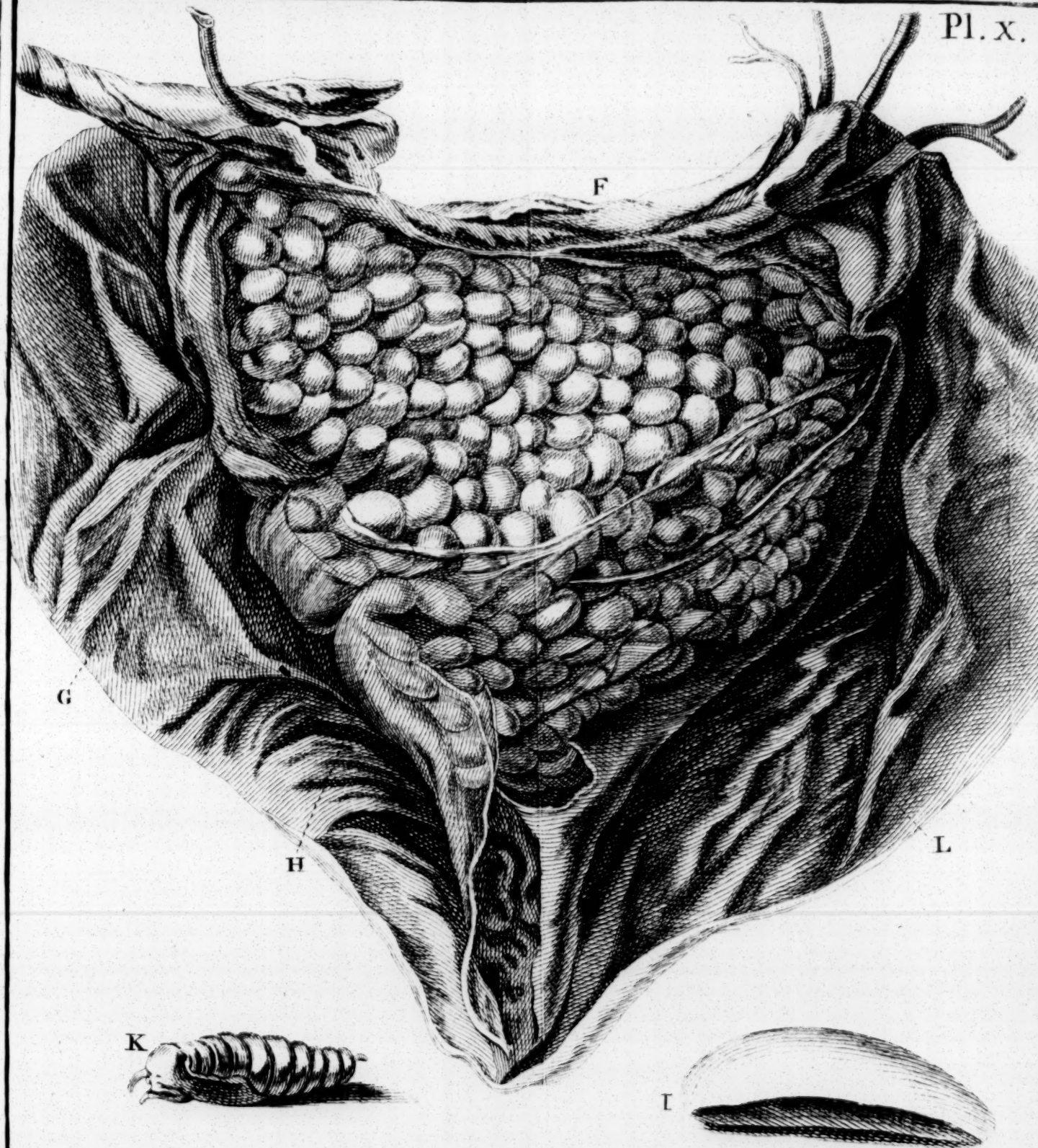
J. Mynde sc.



Pl. IX.



Pl. x.



J. Mynde sc.

filken tow that serves as a stopple to close intirely the cod.

E. The chrysalis drawn out of the cod.

F. A part of the same chrysalis enlarged, in order to make its hooks L, and the *stigmata* M more perceptible.

G. Part of the silk that serves to close the inferior opening of the cod, which is formed in the shape of a tube.

H. The *exuviae* of a chrysalis, which is perhaps of an ichneumon-fly.

Plate 9th.

A. A nest intire, suspended to a branch of a tree.

B. A cod opened lengthwise in order to display the net D, in the midst of which the chrysalis is placed.

C. The posterior part of the chrysalis, enlarged in order to distinguish its hooks E.

Plate 10th.

F. The nest opened along its length, to shew the cods G, their position and arrangement, and the net H which envelopes them, besides the common covering L.

I. The cod.

K. The chrysalis drawn out of its cod.

Memoirs 1749, page 163.

If it may truly be said that the exact and scrupulous manner in which physics are at present

Of the shining of diamonds and other

¹ubstances
in the dark,
by Mr. du
Fay.

present studied, hath made the greatest part of those phantoms in philosophy vanish which appear only in the writings of the ancients; yet it must be owned that we have often taken for fables some singular facts, which were not particularized enough to enable us to verify them, or which appeared to us so little probable that it seemed to incur the censure of credulity even to give them any trial.

Mr. de Reaumur hath already re-established the honour of Pliny on several occasions, and principally in his memoir on dails or dottle-fish, wherein he hath shewn that what we find in Pliny with respect to the light that these shell-fish produce in the dark, is exactly true. *Vid. Vol. 1st, Page 89.* I might cite several other examples of facts, which after having a long time lain under a suspicion of falshood, or at least of an excessive exaggeration, have at last been found in all points conformable to truth; but this detail would not be of much use, and it is better to give a more recent example of these kinds of prodigies, which having been not only doubted of, but even despised, so far that no one would deign to refute them, have at length been verified by experience.

Pliny relates that there are stones in which you see a sort of light or flame to blaze, and that there are others which appear sometimes full of luminous sparkles, *In phlogitide intus ardere quædam videtur flamma, quæ non exeat.*

eat. *In anthracitide scintillæ discurrere aliquando videntur. Lib. 37. Cap. 11.*

Solinus speaks pretty nearly in the same terms of these two stones, *Pblogites ostentat intra se quasi flammæ æstuentes; anthracias coruscat velut scintillantibus stellis. Cap. 37.*

It is not our business at present to discuss what are the stones which these authors speak of: All the commentators of these naturalists have laboured to discover them, but have not been able to agree upon the point; and we shall see presently that this property of giving light is by no means capable of characterising these sorts of stones.

Isidorus of Seville speaks in a more circumstantial manner of the stone, called *ceraunia* or *thunder-stone*, and what he says of it might lead us to some discoveries, if this author had deserved more confidence, and he had not indiscriminately mixed the most extravagant fables with true recitals: He says, *Cerauniorum duo genera sunt, unum quem Carmania mittit crystallini similem, splendet tamen cæruleo, et si sub dio positus fuerit, fulgorem rapit fidereum*; which seems to describe a bluish stone, transparent like glass, which being exposed to the light of the day, is impregnated with the rays of light, and preserves it in the dark. He speaks afterwards of carbuncles, and says, that there are twelve species of them which are luminous in the dark; that what is called *lignis*, being exposed to

the sun, 'or rubbed with the fingers, is said to attract straws or fine threads; that another called *chrysoprase* appears by day of a golden colour, and a fire by night: The rest that he says of it is only an heap of ridiculous marvels; but the reader will observe, by what we have been mentioning, some traces of experiments which we shall give an account of afterwards.

Authors more modern, and incomparably more exact, have spoken very differently, and seem to doubt of all those marvels related by the ancients. Laët in speaking of the anthraces, pyropi,

— *Flammæque imitante pyropo.* Ovid.

and of carbuncles, says that it is commonly thought that these stones shine in the dark like a burning coal, but that he had never discovered this virtue in any stone, although it be confidently asserted by some authors, *Vulgo creditum fuit carbonis instar in tenebris lucere, quod tamen de nulla gemma habemus deprehensum, licet a quibusdam temere jactetur.*

Lewis Vartoman relates that the king of Pegu wears carbuncles so big, and so brilliant, that whoever looks on the king in the dark, sees him luminous, as if enlightened by the sun; but this author speaks of it only upon the report of some travellers; and Boëtius de Boot, who cites this passage from Vartoman,

Vartoman, and another from *Garcias ab hort*, on a fact pretty much alike, doubts whether there be any stones that are luminous in the dark; he doth not say, however, that the thing is impossible, but he doth not believe that any one dares to assert that he hath seen a precious stone luminous in the night, *Verum hætenus nemo unquam vere asserere ausus fuit se gemmam nocte lucentem vidisse.*

Peter de Rosnel, in his Indian mercury, says positively that there are no carbuncles, or stones luminous in the dark, and he believes that what bore this name amongst the ancients, is nothing else than the granate-cabochoon, or the vermeille.

Lewis de Berguen is of the same sentiment, and thinks that the carbuncle, or anthrax of the Greeks, is properly speaking only a granate.

We find nothing hitherto that can incline us to believe that there really exist any stones luminous in the dark; the ancient authors that relate it, say nothing precise enough to determine our opinions; those to whom one should be the most inclined to give credit, do not speak of it from their own knowledge, but insinuate that they doubt of it extremely; and those who speak affirmatively, have fallen into such discredit by the ridiculous fables with which their writings are stuffed, that nothing but an excessive credulity could

induce one to try any experiments upon their authority.

I do not think that I ought to reckon amongst the authorities which are favourable to the existence of carbuncles, that which Arabian authors relate to us in their ingenious romances; they certainly did not design to make us believe what they have said concerning it: But might it not be suspected that something *real* gave a foundation to the marvellous descriptions they have made? At least we see by *this* that the notion of stones brilliant in the dark hath been universally spread, and that there are traces of it in all places, and at all times, where precious stones have been known.

Let us now see what hath been related in favour of these stones by authors that deserve more confidence. Benvenuto Cellini, * an engraver and goldsmith at Florence, says that there are rubies naturally *white*, without their being *so* by the assistance of fire, as is practised with regard to several stones; that the whiteness, however, is not perfect, but livid, and very much resembling that of the chalcedony; that this stone is of no beauty nor use, but that its hardness alone causes it to be placed in the rank of rubies. He adds afterwards, that having promised to say something of the carbuncle, which he calls in Italian *carbonchio*, he shall say only what he knows
for

* Benvenuto Cellini, lib. 1. pag. 10. dell' arte del gioiellare.

for certain. He assures us that in the time of Clement VII, he saw a carbuncle in the hands of a merchant of Ragusa, that this stone was of a pale white, like to that which he had just mentioned in speaking of white rubies, and that it retained within itself a light so admirable that you saw it shine in the dark. *Ma riteneva in se un fulgente tanto piacevole et mirabile, che egli resplendeva nelle tenebre.*

He adds, that this light was not so lively as that of coloured carbuncles, but that he had seen it in the dark shine like a fire that is a little upon the decay. He says afterwards that he had never seen coloured carbuncles, but that a Roman gentleman, very much experienced in the affair of Jewels, had told him that one James Cola, by means of the darkness of the night, had found a coloured carbuncle in his vineyard, that an ambassador from Venice had purchased it for ten crowns of this peasant, and had sold it for an hundred thousand crowns to an emperor of the Turks.

This last history being related by Cellini only as a fact which he had heard, may well be called in doubt, but we cannot say the same thing with regard to what he assures us he was an eye-witness of; and the advantageous manner in which Mr. Boyle speaks, in several places, of Cellini, should have given a greater weight to his testimony; yet

we do not find that this hath engaged any one to make the experiments necessary to ascertain the truth.

I have related in my sixth memoir on electricity the principal phenomena that Mr. Boyle had observed on a diamond which belonged to Mr. Clayton, of whom king Charles II. bought it. *Vide Boyle's works, Vol. 2. pag. 85.*

We often find that those ideas which one would think should present themselves the first, like some coy nymph refuse our solicitations at one time, and come and offer themselves voluntarily at the moment we least expected them ;

Interdum rapere occupat.

Who would not be surprised that Mr. Boyle who had thought of heating Mr. Clayton's diamond by the flame of a candle, upon hot iron, in boiling water, should not have thought of exposing it to the sun ? He imagined no doubt, that heat was necessary to put it in a condition of giving light, and he did not suspect that the heat of the sun could be sufficiently considerable.

I was at first of the same opinion with regard to heat, and after having verified upon several diamonds the experiments related by Mr. Boyle, I plainly discovered that it was not necessary that the diamond should be heated, in order to contract a transient light,
such

such as that which it produces when rubbed upon glass or metals; but I thought that it must be sensibly heated to acquire that permanent and durable light, which subsisted for several minutes when the diamond had been brought near to a lighted candle, or exposed to any other heat. With this view I exposed to a very burning sun-shine for 22 minutes several diamonds, *namely*, one *white* of a very beautiful water, two *yellow* of which one was less coloured than the other, weighing about 60 grains, one of a *rose-colour*, one *green*, and one of an *amethyst-colour*.

I examined afterwards all these diamonds in the dark, and I found only the two yellow *luminous*, but they were *so* extremely, and principally the smallest which was of the deepest colour; they were *so* to such a degree that they enlightened the others which were placed near them, insomuch that it was necessary for me to take these two away, in order to be assured that the others were not luminous: These two yellow diamonds kept their light very preceptibly for more than 12 minutes.

Although every one knows how necessary it is that these experiments be made in the most perfect obscurity that is possible, yet I believe that it will not be useless to mention the *precautions* which it is necessary to take, in order to see these experiments in all their beauty, without *which* I might be suspected

of exaggeration sometimes in the description I give, because without these precautions the same experiments succeed much less perfectly.

It is necessary to continue a quarter of an hour at the least in the most perfect darkness that is possible, before you let the diamonds be brought you which have been exposed to the sun; it requires such a time, in order to let the vibration cease, which the light of the day causes in the eyes, and that the pupil, which had been extremely contracted so as to permit only a certain quantity of rays to pass, whilst you was exposed to the light, might have time enough to dilate itself sufficiently to receive the most that is possible, from objects the least luminous or the least enlightened. There is no one but must have observed several times in his life, that when he goes out of broad day-light, and enters into an obscure place, he sees none of the objects which he easily discerns after having staid there a quarter of an hour, when the eyes no longer feel the impression of the day light. I do not know whether others have remarked the little relation, and the *independency* there is in this respect between the two eyes; for if you close one for a quarter of an hour, and the other continues open, the lustre of the diamonds and of other *phosphori* of the like nature will be seen very distinctly by the eye that hath been closed, whilst the other will not perceive it in the least degree; which
proves

proves that the dazzling caused by broad daylight, and which renders the eye insensible of a weak light, acts upon each eye *distinctly*, and that its action ceases before the union of the two optic nerves. There results from this observation a very great conveniency with regard to the performance of these experiments, for you may close one eye, and yet make all the little preparations that are necessary, and which it is often difficult to leave the care of to another person: This at least is certain, that the observation hath been of great service to myself ever since I made the discovery.

As I had observed in the first experiment I have related, that the diamonds, altho' they had been 22 minutes in the sun, had not acquired any sensible heat, I tried the effect of exposing them to the sun for a less time, I left them there only 10 minutes; there were two large white diamonds, weighing between 70 and 80 grains, two blue, one of which weighed 18 grains, but the other was smaller and deeper in colour, one green, one of a rose-colour, and two yellow; the two white diamonds did not take any lustre at all, nor did the deepest-coloured of the two blue; the green, and the rose-coloured assumed a little splendor; but two small * *carat* diamonds that were set round one of these rings, the two yellow, and above all the large and bright-

* *A carat diamond is one that weighs four grains.*

bright-blue diamond, appeared extremely luminous, and their lustre continued 12 or 13 minutes, growing weaker by imperceptible degrees.

I tried afterwards several other diamonds of various sizes, and of all the colours that I could meet with; I found several of them extremely *luminous*, and others that were not *so* at all. But hitherto I have not discovered any difference in this respect, which could be attributed to the colour alone of the stone, except that the yellow diamonds which I have tried have all been luminous; there were indeed some more luminous than others, but they were not always those whose colour was the deepest. Besides a great number of yellow diamonds, weighing from 8 or 10 to 80 grains, which I have tried, I have had the opportunity of having in my hands about four hundred yellow carat diamonds, and there was not one of them but what was luminous; so that we may establish it as a general law, that all yellow diamonds having been exposed to the light, are impregnated with it in such manner, that they preserve it in the dark for a considerable time.

I did not confine myself, as the reader will imagine, to the examination of diamonds: I exposed to the sun an eastern ruby, a balais ruby, a spinal ruby, an eastern topaz, an eastern sapphire, and an emerald; all these stones were of a perfect beauty and clearness,
but

but none of them took any light, altho' they all emitted light when rubbed in the dark. I afterwards tried the like kind of stones which were much less beautiful, and of which some were *rough*; I was not a little surpris'd to find one single rough emerald, amongst twenty others, that was luminous. I examined it with care, and did not find in it any thing particular to which I could attribute this difference.

This observation determin'd me to try at hazard all the stones of which I have any knowledge; since I saw that nothing could conduct me in the choice of tryals. I had for several years been forming a cabinet of fine stones, jaspers, agates, and several remarkable crystallizations; all these different substances are ranged in drawers divided by compartments, which hath been of great use to me for making at once, and without loss of time, a great number of experiments: I expos'd one after another all these drawers to the sun, and caus'd them to be brought to me in the dark; if I perceiv'd any thing luminous, I took it out of its place, and examin'd it afterwards at leisure.

Amidst this great number of stones, I found much fewer *luminous* than I had expected, and those which were *so*, were not certainly such as I should have suspected of it. That whose light was the most lively, was a pretty large piece of a crystallization mixed with a
good

good deal of topaz, with several little greenish crystals, and a few of an amethyst-colour; this stone weighed half a pound, it had in its greatest thickness no more than about an inch, and became much thinner towards the edges; it was so penetrated with the rays of light, that it was as brilliant in its under part which had not been exposed to the day-light, as in its upper, insomuch that it seemed to be transparent; there did not appear even any difference in those places which had the greatest thickness, they were as luminous as the others, both in the upper and under part; and the light which came from this stone, was such that it enlightened the objects around it to the distance of about two inches; it ceased however to be luminous in about 7 or 8 minutes time.

A piece of lead-ore mixed with parcels of an hard stone, called in French *gangue*, with large cubic crystals of an *aqua-marina* colour, and some small hexagonal white crystals, was likewise very luminous, it was not however so intirely luminous as the preceding, the crystals of the *aqua-marina* had the most lustre, and some of the white crystals had a little, but the lead part, and the *gangue* or white crust had none at all.

A third piece that was smaller, and which was formed of a single piece of green crystal inclining to a blue, intersected only by some cracks regularly disposed, and covered on one
side

side with a crust of little white crystals, and a pale marcasite likewise crystallized, was at least as luminous as the first crystallization I spoke of, and this last has preserved its light a little longer.

There was also a fourth piece of lead-ore, covered in part with a white crystallization, which was luminous towards one of its angles, but whose light was very faint.

I did not try any greater number of these crystallizations, which are common enough and known by every one; we generally give to those that are coloured the name of *primes* of emeralds, amethysts, or topazes, according to the predominant colour. It is very remarkable that these stones, which are for the most part very soft, should be, with regard to the lustre we are speaking of, in the same condition as the diamond; whilst the *hardest* coloured stones, which are commonly called oriental, have no such effect; it may from thence be inferred, that this singular property doth not depend either on the hardness or colour. I have made the same trials on divers other stones, such as rock crystals, talcs, Bolonian-stone before it is calcined, the selenite, different sorts of marble, but none of these substances hath imbibed light; the phosphorus of Berne, which is a transparent stone very white and soft, received light and preserved it a good while; but nothing surprised me more than the *lapis lazuli*: I should certainly never have

thought of making a trial of it, if it had not happened to be in the number of stones which were in the drawers that I exposed successively to the sun; its great opacity, its want of hardness, the metalline veins that are in it, were so many reasons, I thought, for not suspecting that it could be *luminous*; yet it is so, and very considerably, altho' unequally: The most beautiful, or at least the most esteemed, that is to say, what is of a beautiful deep blue, which is the hardest, and takes a beautiful polish, is that which is the most *luminous*; that which is pale, or mixed with white spots, is much less so; yet you discover in it always some brilliant parts, and if you mark with a dot those brilliant parts, you will find that they are such as are of a more beautiful colour than the rest; I have however found some of a very beautiful blue, that gave scarce any light: In general the light is not so lively as that of diamonds, and I have never perceived the least light upon the white spots, no more than on the veins of metal, or of marcasite that you see in it. This experiment on the *lapis* led me naturally to try what effect the *ultramarine* would have; I expected to find it luminous, after what I had observed in the *lapis*; yet it was not so, altho' it continued for more than an hour exposed to the light. I will mention on this occasion that substances which, in the space of a minute, have not taken any light,

light, will receive none, altho' exposed for a much longer time.

The reader will perceive by what we have been relating, how much knowledge still remains to be acquired on this subject; no principle offers itself on which one can rest one's judgment, or even form conjectures: For here are stones of all kinds, of all colours, of very unequal degrees of hardness; in fine, of a very different nature according to all appearances, which produce effects entirely alike; whilst others which seem to have a great conformity together, and even all that our senses can convey, produce effects absolutely different; for I have observed that amongst several diamonds intirely alike, there were some that gave light, and others that were not luminous. It was to these different effects of a stone which appeared the same in all points; or to speak more exactly, of two stones entirely alike as far as we are capable of judging, that I have particularly attached myself; but before I speak of the experiments I made on this subject, I ought to give an account of some facts which regard in general all the *phosphori* of this kind.

I mentioned above that I left no longer than 10 minutes exposed to the sun, those stones of which I was willing to make tryal, and that they had appeared to me as luminous, as when they had continued there a much more considerable time. I tried after-

wards by exposing them not to the sun, but to the bare light of the day; their lustre was at least as lively, and this fact hath a great affinity with what hath been remarked of the Bolonian-stone; *namely*, that it takes more light when it is exposed to the day, and when the sky is over-cast, than when it is illuminated by the sun. I was willing to see what was the time necessary in order that a diamond, capable of becoming luminous, should load itself sensibly with the light of the day; I exposed it at first only *four* minutes, afterwards *one*, and I did not find that this made the least diminution in the splendor of its light; I continued to diminish the time of its being exposed, and I saw that it was intirely usefess to make it imbibe the light so long as I had done at first, for in a second of time, and still less if it be possible, it took a very lively lustre; it is true that the splendor appeared to me somewhat less than when it had continued in the daylight for 8 or 10 seconds; but I cannot be certain that this difference was real; neither did I observe it but in stones less luminous than others.

As Mr. Homberg recommends the exposing the Bolonian-stone to the open air, I was willing to see if that made any difference with regard to the diamonds; I exposed some near the window of a chamber, whose casements and curtains, which were of white
 8 linnen,

linnen, were closely shut ; the diamonds took a light as in the open air ; it is true, that the light was weaker, and proportioned to that which they received.

I exposed to the air, during a second or two, some Bolonian-stones, some belemnites, and common topaz, which had been calcined for more than five years ; I found them all luminous, and it is to be observed that they had been preserved all that time in white paper alone placed on a table in the country, and that they had several times been exposed to the air : From whence it follows, that the first authors, who have written on that stone, prescribed an infinite number of very useless precautions, both with regard to the manner of preparing it, *as we shall mention in the chemical part of this work*, and likewise with respect to the method of preserving it.

Having discovered that diamonds have no need of the immediate light of the day to become luminous, I inclosed several in boxes, to which a coloured glass served as a cover ; that did not hinder them from taking light, and even as quickly, so far as I could judge, as if they had been exposed to the open air ; but this light did not appear to me to partake in any measure of the colour of the glass through which it had been transmitted. All these stones *plunged in water*, and exposed to the light, became luminous, as when ex-

posed in the common manner. The same thing happened when they were plunged in milk, altho' there were seven or eight lines of milk above the diamonds, and that they had continued even the whole night in it, which had given time to the cream to rise and thicken.

I plunged diamonds in ink, and they took no lustre when I exposed the vessel to daylight, without doubt because the rays of light could not penetrate that opaque liquor to arrive at the diamonds. I imagined that this difficulty which the light had to pervade the ink, would furnish me with the means of preserving for some time that lustre, which the diamonds should have acquired; for this purpose I impregnated with light that of all my diamonds which imbibed it the most copiously, and immediately plunged the diamond in ink; at the end of five hours, when the night was come, I drew it out, and looked at it in the dark, it was still very luminous: It is necessary, however, in order that the experiment should succeed, that the diamond be plunged deep enough in the ink, and even that it should be covered with cotton steeped likewise in ink; for I was willing to repeat the same operation, by making use of a little vessel in which there was only liquid ink which covered the diamond about six lines; I left the diamond in this condition for twelve hours, and at the end
of

of that time I found that it no longer yielded any light.

I made experiments likewise by enveloping diamonds in different stuffs, and in papers, to see if their light would be preserved; but I was not able to succeed by this means, no more than by the use of Spanish wax made soft: I had better success in the use of soft and black wax, such as is used in taking off the impression of an engraving; the diamond that I inclosed in it retained light at the end of six hours. I doubt not but it is possible to find some other manner of preserving the light of diamonds, or of other the like bodies, a much longer time; but having other objects which appeared to me more curious to examine, I did not carry my researches further on this subject.

I thought that what deserved more particularly to be examined, was, why of two diamonds, which were in appearance intirely alike, the one was capable of becoming *luminous*, by being exposed to the day-light, and the other did not become *so*? I shall now mention the attempts I made in order to acquire light on this fact, and altho' they have been unsuccessful, yet I ought not to neglect giving an account of them.

I knew that there are many diamonds which are burnt before they are exposed to sale, this commonly whitens them, especially when the yellow or brown tint which

they sometimes have, arises from some portion of the oil that, in polishing them, hath insinuated itself into the little imperceptible cracks which are often to be found on the edges of the stone; the fire burns this oil, and there remains nothing more than very small black points, which diminish much less the lustre of the diamond, than the yellow tint that was occasioned by the oil: I thought that the luminous diamonds might be those which had been calcined, or perhaps those which had not been so, or that it might arise from the degree of heat which they had suffered, in fine that this odd phenomenon might be owing to some cause of this nature. In order to satisfy myself, I picked out ten diamonds, of which four were very luminous, and six were not at all so; I gave them to a diamond-cutter to be burnt, and when he returned them to me I found no alteration in them. I was willing to perform the operation myself, and by applying different degrees of heat; but the success was still the same. I turned my views afterwards to the examination of other stones, which, without being so hard, and so untractable as the diamond, have nevertheless the same properties with regard to light.

We have observed that the *greatest* part of precious stones yield a light when they are rubbed, that there are *some* which become luminous when exposed to the day-light, and that

that *others* become so when they are heated. Among all these stones there are some which have but one of these properties; the ruby, for example, gives a light when rubbed, but when exposed to the day-light it doth not become luminous at all, no more than it doth when heated; others have two of these properties without having the third, the rock crystal is in this case, it doth not catch light from the day, but takes it if it is heated, and yields a light when you rub it. Lastly, some other stones, and the diamond more eminently than any other, have these three properties conjointly. We have seen that Mr. Boyle had discovered two of them, but the most singular * escaped him: And moreover he thought at first that it was only the diamond of Mr. Clayton, which had those two qualities, and was very far from thinking that they were common to almost all diamonds, and to a great number of other stones.

Let us now see what these three properties have in common, whether they are owing to different principles, or whether they may not be considered as proceeding from the same cause, which, in proportion to its force, or its abundance, produces one, two, or all the three properties. I have examined with great exactness, and have found that
the

* Dr. Wall however did discover this property in the diamond. *Philosophical Transactions, abridged by Jones, Vol. IV. Part II. Page 277.*

the diamond receives light from mere heat alone, for I was apprehensive at first that it was the light of the fire on which I caused it to be heated, that rendered it luminous; but I am assured of the contrary, for I caused a silver spoon to be heated, and to be brought to me in a dark place; I had put there a diamond, which, in an instant after the spoon was brought near it, became luminous, it increased its light in proportion as it grew hotter; but this light doth not last any thing near so long as that which is derived from the day-light, for it diminished in proportion as the spoon grew cooler, and intirely disappeared before the spoon was quite cold.

I afterwards boiled some water in a covered vessel, and plunged into it a diamond in the dark, it became luminous before it reached the bottom of the vessel, its light was very lively; but it lasted but a little while, and ceased to be perceived, tho' the water was still hot enough not to let you bear to keep your hand in it: This experiment seems to have been known by Albertus the Great, who says, that the diamond takes light if you pour on it hot water that is very clear: The light which the diamond acquires in these two cases, is of a much less duration than that which it derives from even a very weak day-light; for if you have worn on your finger in the course of the day, a diamond capable of being impregnated with the rays of
light,

light, and you enter into a dark place, about an hour, or even more after sun-set, you will see very distinctly that this diamond still gives a whitish light, insomuch that it requires more time for this light to be intirely diffipated, than is necessary for the eyes to recover themselves from the vibrations and dazzling which the light of the day hath occasioned in them. I am even surpris'd that an experiment so common, and which is made without applying the least attention to it, hath not been remarked by an infinite number of people, for it requires only to go into a dark place after having been expos'd to the air for some time towards the close of the day. It is true that this light is very weak in comparis'n of that which the diamonds take in broad day-light, when all the circumstances are observed, which we mentioned in detail, at the beginning of this memoir.

Having found in diamonds the most resembling each other, this so remarkable a difference, I was willing to see if there was not the same in some other stones; I took sixteen pieces of rock-cry'stal that were cut, and somewhat bigger than pease, there were some of them extremely white, others inclining a little to a yellow, a vinous colour, or a sky-blue; I put them to heat upon a chafing-dish in a silver spoon; I observed a little time after, in carrying the spoon into the dark,
that

that there were some of these stones very perceptibly luminous; I took them out and set them apart; I heated the spoon afresh, and more intensely than at the first, I found three or four which having not been rendered *luminous* by a weaker degree of heat, were now become *so* by this second; I separated them likewise from the rest, and having caused the spoon to be heated again still more, there was one which became luminous; the others did not acquire any light, altho' I heated the spoon as much as it was possible to be without being red-hot. It was useless to carry the experiment further, for if I had made the spoon red-hot, I should not have been able to distinguish the peculiar light of the stones, if they had any, from that which would have resulted from the red-spoon. I examined afterwards by the day-light, those which had been luminous the first, those which had required a greater degree of heat to become luminous, and lastly, those which were intirely impassive of light; and I found that this had no relation to the colour of the stone, that amongst the most luminous there were some extremely white, and some coloured, in like manner as there were amongst those which could not be made luminous.

Having repeated some of these experiments, it appeared to me that after having heated three or four times the same stone, its light was less vivid than at the first, and in
that

that particular I found in them a great analogy with the phosphorus of Berne.

I was willing to see if the action of fire would have a greater effect on the rock-crystal than it had upon diamonds; I picked out two pieces that were nearly equal, the one of which was extremely *luminous* when it had been heated, and the other was not at all *so*, I put them together in a crucible in which I wanted to fuse some silver, I gave the fire necessary for melting the silver, and when it was melted I drew off the crucible, and let it cool in a dark place; I observed with attention whether the pieces of crystal which were half way sunk in the melted silver, would retain their light, or rather their redness, a longer time than the crucible; but no such thing happened, I ceased to perceive them when they ceased to be red, and since that time in vain have I heated them; that which, before this operation, was one of the most *luminous*, hath not been at all *so* since: Here, therefore, we find a real change produced by the violence of fire in this stone; the same thing happened to me in respect of the phosphorus of Berne, the false emerald of Auvergne, and other phosphori of the same nature, when they had been exposed three or four times to a heat much less than that which I had given to my two crystals, they no longer yielded any light.

In

In order to see whether fire would have the same effect on stones which become luminous by being exposed to the day-light, I took a pretty large piece of the phosphorus of Berne, I broke it in two, and put one piece of it to heat in a crucible, wherein I kept it for more than half an hour, carrying it from time to time into the dark, till there no longer appeared any light in it; I thought that having lost thus by fire the property of becoming *luminous* by heat, it would in like manner have lost the property of becoming *so* by being exposed to the light of the day; but I soon found that these two properties were not so closely connected as one might naturally think; for having exposed it to the day-light with the other portion of the same piece, in order to judge better of the difference in case there was any, it appeared to me very nearly as luminous as before. I resolved to calcine it anew, and to give it a degree of heat much more violent; I kept it therefore for three quarters of an hour in a crucible, to an heat pretty nearly such as is requisite for fusing silver; I found after it was cold that it had a little changed colour; for, instead of a *bluish* cast, or *sky* colour, that it had before, it had contracted a light *reddish* tint; which, however, would not have been well perceived, but by comparing it with the first piece from which it had been separated; besides that, its surface was a little

the unpolished, and appeared more opaque, but this opacity did not penetrate to the inside of the stone, as I discovered by breaking it: Upon my exposing this stone to the day-light, it was no longer impregnated with light; thus by a violent fire it was deprived of this second property, as a much less degree of heat had been capable of depriving it of the first.

I repeated this experiment on a * prime of emerald, and on another stone which from its colour might be named *prime d'aigue marine*; both the one and the other being exposed to the day, received a very beautiful light; I heated fragments of both by a very brisk fire, till that being carried into the dark they would no longer yield any light; but they still continued luminous after having been exposed to the day, and I was not able to strip them of this second property, but by giving them a degree of heat much more violent, and to such a degree that they were in part vitrified at the bottom of the crucible. It is certain therefore, that these two properties have a great conformity together, since you may take away both the one and the other by means of fire, but that they differ in this, that the one is destroyed by a much less heat than the other.

If

* *Prime of emerald is a precious stone half transparent and half opaque, called by the ancients chrysoprasius; it is considered as the mother of emeralds.*

If I was not able to succeed in the like experiments on the diamond, the reason perhaps is, that it would require for that purpose the fire of a glass-house, or even a fire still more intense: Perhaps all diamonds, as well as several other stones, have *originally* this property, and that they are only such as have been in vulcanos, or suffered the torture of subterraneous fires, which have been deprived of it; the reader will perceive that this is but a slight conjecture, but as it is not destitute of all probability, I thought I might venture to mention it.

By following the analogy of different phosphori, I had reason to think that the Bolonian-stone, the belemnites, the gypsum, the phosphorus of Balduinus, and all the others of the same nature, would become luminous by heating them, and that by a violent calcination they would lose their quality of *phosphorus*, in like manner as those of which I have been giving an account; but experience hath made me see the direct contrary, for in the first place none of these phosphori receive light from mere heat; and in the second place the most violent calcination was not able to take away the property of becoming luminous, when they were exposed to the day-light. What is to be concluded from these apparent contradictions, except that this subject is yet too novel, that we are struck with certain apparent conformities, which in the main are
very

very remote, and that it is only by a great number of experiments combined in a vast variety of manners, that we shall be able to find any plausible explication of facts so singular?

As the greatest part of stones that are luminous, whether by heat or whether by exposition to the day-light, yield a light when they are rubbed upon stuffs of silk or wool upon glass, earthen-ware, metals, &c. tried if these same stones, after having been *deprived* by calcination of the two first properties, would be *so* likewise of the third; but I have not been able to make the experiment on any but the phosphorus of Berne; because the greatest part of the others which I tried, burst into splinters, and were reduced to very small fragments by calcination. I rubbed therefore the phosphorus of Berne upon delft-ware, and upon glass, it gave a light, in the same manner as it did before calcination; which seems to prove, that this third manner of yielding light hath no relation with the other two: Perhaps this is the only one which is a-kin to electricity, and what may further favour this conjecture is, that glass, China-ware, precious stones that are coloured, and some other substances, have not the two first properties, but only the third, and that these are the bodies which are acknowledged to be the most electrical.

I am far from giving these observations as certain principles, for in the prodigious number of substances which have one or several of these properties, there may be found some that would intirely disfrange my hypothesis. I see yet only a confusion of facts, the explication of which appears to me almost impossible, and which seem even contradictory; for of two diamonds intirely alike to the sight, the one is extremely luminous, the other not at all so; the topaz of Auvergne is impregnated with the rays of light, whilst those of the east, of India, and Bohemia, receive no such glorious burden. It is the same with regard to the emerald of Carthage, and that of Auvergne, it is only the last which takes light from the day, or by the heat of fire. The Bolonian-stone, and an infinite number of others, continue luminous, notwithstanding a violent calcination; on the contrary, the phosphorus of Berne, the primes, &c. lose this property by calcination: These last, in like manner as the diamond, preserve always their faculty of being impregnated with light, insomuch that it seems to be inexhaustible; yet the Bolonian-stone, the phosphorus of Balduinus, and others of the same nature, lose it by little and little, and so much the more speedily, as they are more frequently exposed to the daylight. The reader sees how many different effects are in substances which one would think alike,

alike, and how many different circumstances in those which appear to produce the same effects! Let us content ourselves, for the present, with making experiments, collecting facts, and examining well all the circumstances of them, and all their relations; and let us wait till time, and assiduous labour, have discovered to us the connection of these facts with one another, and the principles from whence they are derived; if these principles are not of the number of those whose nature it is not possible for us to know, or to form an exact idea of,

Caliginosa nocte premit Deus.

Memoirs 1735, pag. 347.

About the year 1676, Mr. Picard in carrying his barometer from the observatory to St. Michael's gate in the night time, perceived a light in that part of the tube where the mercury was in motion; this phenomenon having surprised him, he communicated it immediately to the learned; and those who had barometers having examined them, there was not one found that occasioned light. He thought at first that it was owing to the mercury, which had been revived from Cinabar; but having given some of the same to Mr. de la Hire, who wanted to make the experiment, the barometer which Mr. de la Hire mounted with this mercury, hath hitherto been without any sensible effect.

Of the shining of the barometer in the dark.

After the death of Mr. Picard his barometer was dismounted; Mr. de la Hire having filled it again with the same mercury, he remarked no light in it. It was about this time that Mr. Cassini perceived that his barometer began to give a light, which it hath always continued to do even to the present time.

Mr. de la Hire by frequently handling that of Mr. Picard, which he had recomposed, found at length after some time that it began afresh to be luminous, as before: Yet some years after, it lost again this virtue, although Mr. de la Hire was sure that no one had touched it. He thought then that the matter which occasioned the light, and which was included in the mercury, was either wasted or dissipated, and that he could not expect to restore it; at last, after having been dismounted and remounted, the barometer gave light afresh when it was shaken, and the first spark that appeared was the most lively of all that Mr. de la Hire had seen it produce: It continues still to give light, but less than at the first. That which appears singular in this barometer, is that it hath lost of itself and resumed afterwards at different times the property of being luminous.

Mr. de la Hire hath remarked also, that there is a considerable difference between the *light* of his barometer, and *that* of Mr. Cassini's; in his own the light fills pretty equally all the *vacuum* of the tube at each vibration; whereas

whereas in that of Mr. Cassini it seems attached to the surface of the mercury, from whence it diffuses itself through the tube.

History Tom. 2. anno 1694, pag. 202.

This phenomenon excited the curiosity of most of the philosophers in Europe, and set them on the pursuit of some ignis fatuus of an hypothesis to solve it, by which they bewildered themselves and others: The famous Bernoulli distinguished himself in this learned error, and shewed the world how vain are all inquiries in Philosophical subjects, unless nature lead the way,

Matre Dea monstrante viam;

To this queen of beauty the painter, poet, and the sage, should pay an equal homage, the sole measure of perfection in every art, the source of truth in every science.

It is necessary to advertise the reader, that there are some articles, from which I have omitted to give any extract; because they are either founded on a precarious hypothesis; or they are subjects that have been much better handled by writers of our own; such for instance are the rise of liquors in capillary tubes by the learned Dr. Jurin, and the theory of the tides by the incomparable Sir Isaac Newton,

Qui genus humanum ingenio superavit, et omnes

Restinxit, stellas exortus uti ætherius sol.

Conclusion of the General Physics.

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